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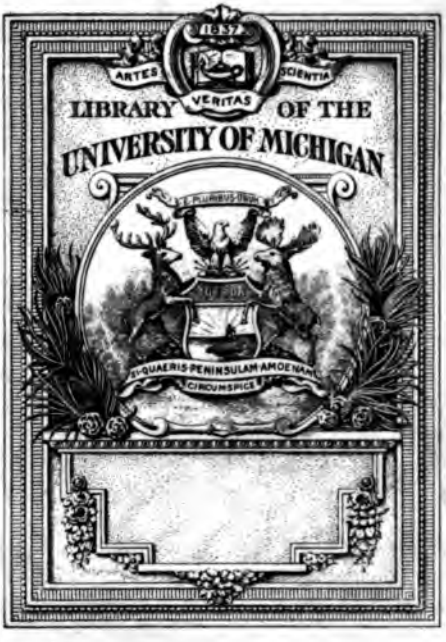
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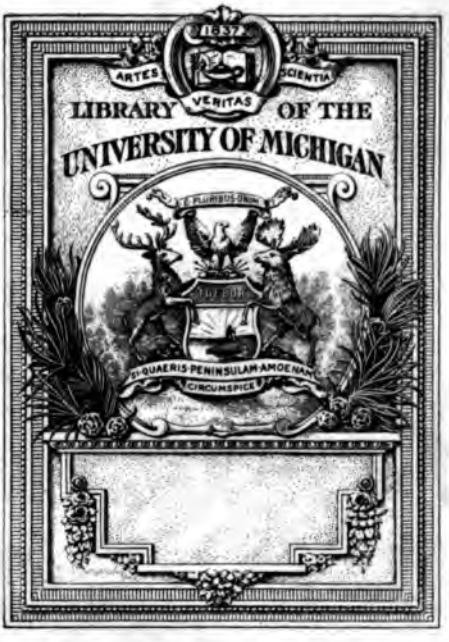
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THE ASCLEPIAD.

415-12

A Book of Original Research and Observation

IN THE

SCIENCE, ART, AND LITERATURE OF MEDICINE, PREVENTIVE AND CURATIVE.

SECOND SERIES.

1892.

BY

Benjamin Ward Richardson, M.D., F.R.S.

VOLUME IX.

TERAR DUM PROSIM.

London:
LONGMANS, GREEN, AND COMPANY,
PATERNOSTER ROW.

Printed by Hazell, Watson, & Viney, Ld., London and Aylesbury.

To

PROSPER DE PIETRA SANTA, M.D.,

LAUREAT DE L'INSTITUTE (ACADÉMIE DES SCIENCES),

AND

DOYEN OF SANITATION OF FRANCE,

THIS NINTH VOLUME OF "ASCLEPIAD" IS INSCRIBED AS A

TRIBUTE OF TRUE ADMIRATION OF THE BEST OF

WORK WELL AND FAITHFULLY DONE,

AND AS A TOKEN OF SINCERE AFFECTION,

BY

HIS FRIEND OF FORTY YEARS,

THE AUTHOR.

PREFACE

TO THE NINTH VOLUME OF THE "ASCLEPIAD."

THE great importance that has been attached to the subject of chloroform administration during the past year has led me to republish, with commentaries, a course of lectures on the subject delivered in the years 1869-70. I am glad to find they have been satisfactory to large numbers of my readers.

I am also pleased to find that the enlargement of the volume has been a source of gratification to subscribers, and to note the increasingly kind appreciation of the work on the Continent of Europe, as well as in the Colonies and in the United States of America.

BENJAMIN WARD RICHARDSON.

5, MANCHESTER SQUARE, LONDON, W.,
December 15th, 1892.

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Thomas Willis

Autotype of an engraving by G Vertue in 1742 from a painting then in the possession of Browne Willis Esq. F.M.R.

Asclepiad No. 33 Vol. IX First Quarter 1892.

Thomas Willis

Autotype of an engraving by G. Vertue in 1742. from a painting then in the possession of Barnore Willis Esq. BMR.

ON THE CAUSE AND PREVENTION OF DEATH FROM CHLOROFORM.

INTRODUCTORY NOTE.

IN the years 1869-70 there was much professional anxiety owing to the large numbers of deaths then occurring from the administration of chloroform. At that time my usual courses of experimental and practical medicine were in regular progress, and, in response to the wishes of the class, the subject of "Death from Chloroform" was selected for three lectures, with the necessary demonstrations. The lectures were delivered on the dates of February 1st, March 1st, and April 5th, 1870, and were reported in the *Medical Times and Gazette* on May 14th and 20th and July 23rd of the same year. During the last two or three years the anxiety respecting death from chloroform has been renewed, and since, on reading the lectures over again, I find them as applicable to-day as they were twenty-two years ago, and anticipatory of much that has been recently advanced, I propose in this new volume of ASCLEPIAD to reproduce them, following them out largely in their original form, but adding to them some notes on new experiences that have been learned during the interval that has passed away.

I shall retain also the original style of lecture, as most appropriate to the demonstrations by which the lectures were originally illustrated.

LECTURE I.

[DELIVERED ON FEBRUARY 1ST, 1870.]

GENTLEMEN,—I am following, I believe, your own wishes in taking up death from chloroform as a subject for the present course of lectures. The time is fitting for a careful study of the important question before us, for the deaths from chloroform seem to be—I do not say they are—seriously on the increase, and the hearts of the boldest men are in some fear whenever they summon chloroform to their aid. Certain ascertained facts respecting the cause of death from the agent are insufficiently known; certain new facts admit of being added to what are known already. Finally, certain conclusions require to be arrived at as to the possibility or impossibility of making chloroform so safe an agent that it shall maintain its place as the chief instrument for the production of general anæsthesia.

We have work enough on our hands if we include in our labour some of these various subjects. Happily for us in this place, secluded from the multitude, we can study the difficulties we would like to solve, with patience, quietude, exactitude. We can read, in short, from Nature, who, as Walter Savage Landor tells us, reveals herself not to the crowd nor by sound of trumpet, but to those who inquire with the simplicity of children, and look earnestly in her face for reply.

In the outset of our work I think it best we should honestly admit this truth: that, whatever our admiration may be of the scientific advancements which have in our day been made for the relief of pain during surgical

operations, we are bound to season the admiration with the disagreeable knowledge that the blessing we confer on humanity, when we resort to the application of general anæsthesia, is not unmixed with danger and sorrow. At present we are forced to know, for example, that the administration of this, the most common vapour employed for anæsthesia—chloroform—is attended with a certain fixed mortality. We are bound, therefore, to consider whether that mortality is necessarily to remain; we are bound to inquire if the mortality is an essential part of the administration; and, if it is, we are bound further to ask whether the general value of the agent counterbalances its dangers and its fatality. It has been urged that if any other medicinal substance than chloroform had caused as many deaths, it would long ere this have been ruthlessly expunged from practice. The assertion is true and untrue: true if the argument were that a thing of doubtful usefulness or of limited usefulness produces a certain evil; untrue on the argument that a thing of certain and extended usefulness produces no more than an occasional evil. But chloroform is of certain and most extended usefulness, with occasional evil; therefore the argument against chloroform, when carried to the expulsion of it because of its evil, is too extreme.

Behind all there remains the inquiry whether we ought to accept the necessity of danger from general anæsthesia at all. Can we avoid every danger and supply every good? Perhaps we cannot; we ought nevertheless to try so to do, and perchance we may succeed. In endeavouring to avoid danger we have two lines of research before us. We may inquire whether we can so reform the anæsthetic process altogether as to ensure success; we may inquire whether we

can so master the effects of anæsthetic agents that we can use *any* with perfect safety, and specially *that* one, *chloroform*, with which the world and the profession are so familiar. I, for my part, have trodden both these lines of research. I have introduced new methods, which, I have hoped, would conduce to safety in anæsthesia. I have studied much to make local anæsthesia a ready and perfected process, and on the whole I have reason to be satisfied with the results which have fallen to me. In science, however, it is quite hopeless for any man to harbour a prejudice or ignore natural truth; and so I am open to confess that, however we may yet perfect the only perfectly safe—I mean the *local*—method of abolishing pain, we shall still often require a general anæsthetic. The demand for a general method is, truly, psychical rather than physical, but it is a strong demand. General anæsthesia differs from local, in fact, in this one particular, and in none other: that it removes mind and body together out of the reach of pain; in this lies its strength. The majority of persons having to submit to a surgical operation prefer to have no knowledge of the operation whatever; they pray us to make the mind insensible as well as the body, and to let both lie unconscious even of immunity from all suffering. I have seen strong men who have had no sentiments faint during an operation when pain was locally removed, and have heard them afterwards affirm candidly that they felt no touch of pain, and yet could not help being faint. Now this is a disagreeable truth, and I warrant that if I or any one else should so clearly learn how to remove pain that we could make large sections of the body insensible and utterly dead for a time, we should still not get over the phenomenon of mental faintness until we extended the insensibility to the organ of the mind

itself and for a moment effaced the tablet on which the impressions of mankind are written.

I keep then in the present course to general anæsthesia, and for the sake of avoiding any confusion I keep to the part of the research above mentioned—viz., research to find whether we can make every anæsthetic safe, and thus be able to retain chloroform as the narcotic on which we may at all times, and for all times, confidently rely. I would myself far rather succeed in this research, if it were possible, than discover a substance which should displace chloroform, inasmuch as the discovery of means of safety of the kind named would unfold the richest treasures of medical science and art. If we, by our knowledge, can take a living animal, put it into such deep repose, inertia, that it shall represent in its negative condition blank death, so that persons generally would bury the animal as something dead; and if from this state we could proceed leisurely to set the animal in motion again, as we set a silent and motionless clock in motion when we give it renewed force from our hand, then we should accomplish far more than if we had discovered a thousand new individual remedies. We should reach then great principles in relation to living action. This restoration of motion in inert organic matter that has lived will be the ultimate and grand result of physiological learning. It is wonderful what is done already towards the result; and it is obvious that every advance we make in studying the extreme action of chloroform, and in discovering modes of recovery from such extreme action, serves the double purpose of making the administration of one particular medicine safer, and of opening the way further towards the discovery of suspending and restarting, at will, animal organic motion. We will therefore proceed with the

study of death from chloroform solely, and, that we may do so more surely, we will proceed leisurely, taking up first the all-important point what is the actual amount of danger that has to be met.

RATE OF DEATH FROM CHLOROFORM.

In the commencement of our inquiries it is well to consider what is the actual proportion or rate of death from the administration of chloroform. To arrive at correct results on this subject is a difficult task, for many deaths have not been recorded, and there is a natural diffidence on the part of individuals and of the managements of public hospitals to speak of fatal cases. When a practitioner has administered chloroform a very large number of times without having met with a fatal case, he is often led to underestimate the danger, and to ascribe his success to some particular mode of administration. I am satisfied this is not reliable experience. I can myself say that I was never present when a human being died from the administration of any anæsthetic, and I have been engaged in the study and practice of general anæsthesia ever since the introduction of the practice. But the favourable evidence I should give on my experience would be worse than worthless, for at least three times in my life some accident has prevented me from being present at cases which have ended fatally, and which would have ended in the same way had I been the administrator. Only recently I was conversing with a surgical friend on this subject, who was not himself an administrator of chloroform, and who told me that he had been present at not less than six fatal administrations. Now his experience had not been so large as mine, yet had it been six times as large, it would have been six times

more unfavourable. I have by me reports, very brief in each case, but most important, of over four thousand administrations of chloroform by the late Dr. Snow, all drawn up by his own hand; the whole record is free of a fatal case; there are some very close escapes of death, but no obvious direct death from the narcotic. This, again, is a remarkable experience, but if I were to compare it with the experience of two other equally careful administrators who have followed faithfully the method and practice of Snow, theirs would differ full half as much as that of mine, and of my surgical friend of whom I have spoken.

Here before us is a still more valuable series of facts. When I was engaged in writing the "Medical History of England" for the *Medical Times and Gazette* in the years 1864 and 1865 I visited in turn eight hospitals—viz., Norwich, Lynn, Stafford, Wolverhampton, Newcastle-under-Lyne, Brighton, Birmingham General Hospital, and Birmingham Queen's Hospital. From the books of these institutions I collected, personally, the number of administrations of chloroform in each institution from the first, in 1848, and before I arrived at a death I recorded no less than 17,000 administrations. Now, one death in 17,000 cases reduces the mortality to a nominal value, and if this experience were supported by all experiences we need trouble ourselves little for any better agent than chloroform. But mark the result of the five years' subsequent experience in the very same institutions. Since 1864 there have been in these hospitals 7,500 administrations, with six deaths, or one death in every 1,250 cases. After I had visited the hospitals above named in 1864 I visited in the same and the following year six other hospitals—viz., at Lincoln, Bath, Oxford, Cambridge,

Reading, and Nottingham. In these I collected the facts of 7,900 administrations from the year 1848, with a result of three deaths, or one in 2,683 cases. In these same hospitals in the subsequent five years there have been 2,762 administrations, with a result of one death.

If, finally, in relation to these large hospital statistics we put all the facts together, we find that in the twenty-one years from 1848 to 1869 inclusive, in the thirteen hospitals named, there were 35,162 administrations of chloroform, with a proportion of eleven deaths. I believe this to be the largest reliable series of cases of administration as yet collected, and I know it to be just. Doubtful cases of death from the vapour there were none, and in every case a qualified and competent practitioner was the administrator.

When from individual and general hospital experiences we pass to the experiences of particular hospitals, we often find the widest differences of results from chloroform administration. Some hospitals, like some individuals, are fortunate, some unfortunate. There are before me the statistics of two hospitals so alike we might call them twins; they have the same average of patients, the same average number of administrations a year, the same precise length of experience. In one of these there have been 1,575 administrations without a death; but in the other the mortality has been at the rate of one death in 525 cases. I could multiply these illustrations were the labour necessary. It is not necessary. My preliminary purpose is fulfilled if I have proved that, in face of the fact of frequent runs of so-called "good luck" to particular men, to groups of hospitals, or to particular hospitals, there is, under the most favourable aspect of chloroform, a given mortality

which up to this moment seems to be a necessary mortality, just as there is a mortality from accidents and acute diseases like fevers. The mortality is, moreover, considerably greater than is made known, for cases occur which are not recorded. To compute favourably from the facts given above the rate of mortality stood as one in 3,500 administrations of chloroform; but I think it, generally, is greater, and that one death in 2,000 to 2,500 administrations would be nearer the truth; yet even at the lowest rate we employ no other narcotic that approaches chloroform for danger.*

THE CAUSE OR CAUSES OF MORTALITY FROM CHLOROFORM.

With the acknowledgment of a certain high mortality from the administration of chloroform staring us in the face, we are obliged to ask what is the cause, or what are the causes, of the penalty. Recovery is the rule; why is there ever an exception to it? We must inquire here, step by step, excluding as far as possible all suspected causes that prove, on inquiry, of no effect, whilst omitting no cause, however trifling it may appear, that seems to lie under suspicion. What then may be a cause, or causes, under suspicion?

(*a*) The cause may lie in the physical or mental condition of the patient subjected to the chloroform.

(*b*) The cause may lie in the immediate surroundings

* Within the past three years—1889-92—there has been a difference of computation on the mortality from chloroform, the general impression being that the above estimate of one death in two thousand to two thousand five hundred cases—an estimate once considered as too high—is really too low. After reading the arguments that have been ably adduced, I am not inclined to move from my old position. Subject to newer and better correction, it is, I believe, so near to the truth as to be, approximately, and for practical purposes, a normal figure.

of the patient, and especially in those that are atmospherical.

(*c*) The cause may lie in mode and skill of administration.

(*d*) The cause may lie in impurity of the chloroform employed.

I propose to follow up our researches under these separate heads. If the answers should be doubtful or negative, it will then be necessary to inquire whether the danger lies in some inherent fault in chloroform itself as an anæsthetic, and whether that fault is irremediable. This will lead to the study of means of prevention of danger, and of restoration from danger.

CONDITIONS OF THE PATIENT, PHYSICAL AND MENTAL, AS CAUSES OF DANGER.

Can unusual danger be predicated from the condition of the patient? Here is a point I am constantly consulted upon. But last week I had two cases before me for advice on this one subject. Well, while I dispute altogether the dogma that whenever an operation is justifiable chloroform is justifiable, because there are numbers of useful operations for which it is unjustifiable, I am not prepared to bring any dogmatical rule against chloroform founded on the condition of the patient. Sometimes persons die under chloroform who have no appreciable disease whatever before death—no disease, that is to say, which the most perfect diagnostician could put his finger on and say there is cause of anxiety from the presence of disease. In the only case where I thought life had been extinguished by chloroform (happily I was wrong) the patient was as perfect a specimen of a healthy body I ever remember to have seen. It was the case of a young lady who had from

her birth been in health; she was free from the least affectation. She came under the influence of the narcotic with the utmost ease; the operation—the extraction of a tooth—was quickly and skilfully performed, and a full minute after the chloroform had been withdrawn the breathing was tranquil, the pulse steady, the countenance natural. Never was a moment when there was less cause for anxiety under the same circumstances, when suddenly there were lighted up all the phenomena of death—the respiration became imperceptible, the heart inaudible, the limbs flaccid. Fortunately, the heart, feeble as it was, continued, and, after three hours of anxious waiting, the recovery was completed. An hour or two previously I administered chloroform to an aged lady for a much longer time, and witnessed no symptom whatever of danger. The same specimen of chloroform (I mean, of course, a specimen out of the same bottle) was used, the same mode of administration was followed, the same hand administered; and yet how different the results. They were results which I, at all events, had not the sagacity or knowledge to predict at the time they occurred, in 1860, and which now would be as far from me as ever. *

There is a fact that I have learned, moreover, in experiment, bearing on this same subject, and which deserves notice. I have occasionally had two animals of the same age, breed, and condition in this glass chamber breathing at the same time and for the same time the same atmosphere of chloroform and common air. They have become narcotised by the vapour, and when I have proceeded to remove them I have found one dead and the other alive and apparently free of

* Continued observation up to this year—1892—confirms me in the same opinion.

danger. This is a result which, I think, could not possibly be foreseen. *

We turn from natural to diseased conditions of body, and the difficulties continue. I believe I know of one condition of body which may be diagnosed as specially dangerous for chloroform, and there my knowledge is brought to an end. This unfavourable condition is present when careful diagnosis shows the existence of a weakened and dilated right side of the heart, with enlarged hæmorrhoidal veins, varicose veins of the lower extremities, and large, full, yet not tense, veins in the other parts of the body. In the body thus circumstanced we may be certain that the right side of the heart, which is the most important organ to be sustained in action under chloroform, is much enfeebled, and may readily succumb if subjected to congestion.

Beyond this point of diagnosis, I repeat, I know nothing definite. When I was engaged in the practice of the administration of chloroform I was careful to make diagnosis of disease before administration of the

* I have observed this event occur, especially in the early working of it, on a large scale, in the lethal chamber. The same kind of animals —dogs—scarcely ever die in the same lethal atmosphere at the same minute, although the conditions are the same. All fall asleep nearly at the same moment, but once asleep one may live thirty and even forty minutes after all the rest are dead. In one instance an animal continued breathing an hour in the midst of a number of others that had been dead an hour and were far advanced in rigor mortis. This animal would have rallied back to consciousness had it not been subjected to a new charge of the narcotic vapour. The only practical mode of meeting this singular immunity of particular animals is to submit all to an overwhelming dose of the vapour from the first, and even that does not make the period of death absolutely the same all round. Under the surest system five minutes have to be allowed for the final event, and even then some animals continue to breathe two, three, or four minutes after the majority have entirely ceased to live.

narcotic; and, on referring to the facts, I find I administered it in the presence of the most extreme forms of organic disease: in phthisis pulmonalis in various stages; in cancer in various stages of the malady; in chronic bronchitis, asthma, and hydrothorax; in cases of mitral disease, hypertrophy, and dilated aorta; in epilepsy; in idiocy with epileptic disease; in various forms of dropsical effusion; in paralysis and acute mania; in some acute diseases, such as croup and pertussis; and, in disease of the kidney with albuminous urine. In not one of those administrations did it appear that the danger of administration was in any sense increased. But my own experiences are small when compared with those which Dr. Snow has written in his book of recorded cases, and he came to the same general conclusion, expressed above,—a conclusion affirmed, I believe, by all who have become practically conversant with the subject.

Some observations have been made relative to the influence exerted by chloroform on persons of the different sexes and on persons of different stages of life. The evidence in respect to sex is affirmative of no special fact indicating that either sex is subject to peculiar danger, but in the matter of age it has been affirmed that the young are exposed to less danger than the old. It is true that in the lower animals it is easier to resuscitate from apparent death a young than a middle-aged or aged animal; and it has been assumed, on the statistics of fatal cases in the human subject, that fewer deaths have been recorded in the young than in those more advanced in life. But, for my own part, I am unable to come to any satisfactory conclusion; for I find that some young inferior animals are as easily killed as older animals, and the statistics

relating to the human subject are faulty in the main point, inasmuch as no comparison has been made of mortalities in a given number of administrations. When we consider how many more adults and middle-aged and aged persons, massed together, are made to take chloroform than young children, we cannot reason on the subject; in short, a sufficient number of deaths from chloroform has not yet been accurately tabulated to allow of any safe inference being drawn on the point. We may, therefore, leave this question of age and its influence an open question, and wait for further information. Lastly, the effects of mental influence and habit have been considered in relation to the action of chloroform on the body. Snow made a very sound observation on this matter which I have often seen proved true; it is that persons of strong muscle and athletic power are brought under the influence of chloroform with much more difficulty than are persons of weak, sensitive natures, and of refined and highly cultivated minds. It does not, at the same time, follow that the danger in one of these classes of cases is greater than in the other, for those who come readily under the influence of chloroform are subjected to a smaller dose than the stronger class, and this compensates for what might otherwise be a greater danger.

On the whole, the only diagnostic I could give as warning the practitioner of exceptional danger from chloroform is the diagnosis of dilated and weakened right heart, with the other attendant symptoms already explained. If I have one further misgiving in respect to dangerous cases, it relates to cases of kidney disease with albumen in the urine, and disposition to uræmic sleep. Here, however, the misgiving is based on theoretical reasoning alone, is greatly negatived

by the knowledge we have acquired from practical observation, and perhaps had best be put aside.

From these physical conditions of body there does not seem, then, great cause of danger under chloroform. There are some who say that asthenia and enfeebled heart should be added to conditions of danger. I have never seen any sound evidence in support of this view, and Snow would never entertain it. "If," he maintained, "a patient is considered sound enough to bear an operation, he is certainly sound enough to bear chloroform." Dr. Sansom has also a very good observation on this point. He remarks that "persons who are in health and are about to be subjected to some minor operation are put under chloroform with more difficulty" —and he might truly have added with more danger— "than those who by long illness and suffering are resigned to their fate, and who, prepared to meet the worst, have, to some extent, lost the sense of fear." Few better observations of a practical kind have ever been made.

Apart, however, from specific detectable conditions, there is a general condition of body, a condition of nervous excitability, which is unfavourable to an extreme degree. Persons of nervous lymphatic temperament are sometimes met with who have no special conditions of disease, but who feel shock from everything more easily than the rest of mankind, and who are "distilled to jelly" by the act of fear from comparatively slight causes. These are in danger before ever they inhale a full breath of the vapour of chloroform, and we might say of them that they would die under the impression produced by any anæsthetic whatever did we not know that some other gases or vapours—nitrous oxide gas, for the most striking example—are breathed by them, up

to insensibility, with impunity. These consequently are unduly susceptible to vapour of chloroform, and die, as we know too well in some unhappy instances, from the smallest effective portions of the vapour. Here we may say we have a condition of danger that is knowable; and when I come to the consideration of the phenomena of death from chloroform it will form an important subject of study. *

CONDITION OF SURROUNDING AIR AS A CAUSE OF DANGER.

The condition of the air in respect to moisture and temperature is of great moment. In the first place, if the air be surcharged with moisture at any temperature the introduction of the narcotic vapour into the blood and the condensation of the vapour in the blood will be impeded, and the stages or degrees of narcotism prolonged. In like manner recovery from the narcotism after the vapour is withdrawn is slower in a moist than it is in a dry air. And, lastly, if in an air saturated with moisture there appear during administration any symptoms of failure of the circulation, the danger is much greater than it would be were the air dry, since with failure of the circulation and loss of radiation from the open moist surfaces of the body there is a quick condensation of fluid, and accumulation of fluid upon open surfaces. The cold sweat which settles on the face when a person faints is not the sweat of natural excess of water thrown off by the body, but is the water, which ought to pass off naturally by the skin, condensed on the skin. The sound people commonly call the "death-rattle" means the same phenomenon—the phenomenon of accumulation

* These are the susceptible persons I have classified as belonging to the *Morituri* (see ASCLEPIAD 24, Vol. VII., pp. 1—20).

of water in the air-passages, from cooling. Under chloroform there is a natural tendency to reduction of animal temperature, and during exhaustion under chloroform this reduction is most pronounced. Hence, unless the air be dry, the accumulation of fluid on surfaces like the bronchial and vesicular surfaces of the lungs is a ready process, and the mischief which may follow, trifling as it may appear, is most serious.

The observation herewith made has been forced upon me often in experiments for the resuscitation of animals after the cessation of respiration under chloroform. In setting up artificial respiration I have sometimes found the operation so free and so effective, I have wondered at the result as much as the lookers-on. In other cases in setting up the artificial respiration I have found the operation at once impeded, and even rendered abortive, by nothing more nor less than the presence of water in the air-passages. In such cases, as we exhaust and refill the chest with air, we can hear the cause of difficulty in a moist fine bronchial râle; and, when the bronchial surface is exposed after death, the smaller tubes are found filled with frothy secretion.

As to temperature. In previous lectures I have carefully demonstrated the effect of temperature on the process of inhalation; I need not do more now than touch on the subject. Suffice it to repeat the following facts: (1) that a high temperature quickens the change of chloroform from the fluid to the gaseous state; (2) that it quickens diffusion in the air; (3) that it quickens the process of condensation by which the vapour is received into the blood; (4) that it quickens the narcotic action; (5) that it quickens recovery from narcotism when the narcotic is withdrawn. Concerning these points the second and third

named are the most important to remember; in fact, they govern our knowledge on the other points. Snow constructed from his own experiments a most useful table, which is here copied. In this table it is shown that a volume of 100 cubic inches of air will have diffused through it certain given measures of chloroform vapour at certain temperatures, the air being assumed as a constant quantity of 100 cubic inches.

Temperature Fahrenheit.	Cubic inches of air.	Cubic inches of chloroform.
40°	100	7
45°	,,	8
50°	,,	9
55°	,,	11
60°	,,	14
65°	,,	19
70°	,,	24
75°	,,	29
80°	,,	36
85°	,,	44
90°	,,	55

I have myself further shown, by direct experiment, that the rate of diffusion and of condensation of chloroform in the blood is so governed by the temperature of the air that the difference of 30° Fahr. makes a difference of six minutes in the time required for the production of the third degree of narcotism; that the second degree, the degree of muscular excitement, is greatly prolonged by a cold atmosphere, and that the danger is increased by cold.

The safest average temperature for inhalation is from 60° to 70° of Fahrenheit's thermometer. A temperature below 60° is too low; and a temperature above 70° is not too high, seeing that the circulation is best maintained, under chloroform, at high temperatures.

EPIDEMIC NEUROPARESIS.—INFLUENZA.

THE LESSON OF SYMPTOMS.

THE more closely I have watched the phenomena of what is called influenza, the more certainly I have become impressed with the conviction that the symptoms as a whole are identical with certain of those defined and classified in my paper read before the Medical Society of London on Monday, December 12th, 1887, and published in ASCLEPIAD, No. 7, Vol. V., pp. 1—22, to which I gave the name of *Pulmonary* or *Pneumo Paresis*. At the time I wrote that paper I looked upon the conditions named as idiopathic purely, running a course less pyrexial and more prolonged than the epidemic disease which has recently been amongst us and in all parts of the world; but, as I have analysed symptoms one by one, I have been led by force of observation to the view that the only difference between the two forms of disease is that the present widely distributed disease is more rapid in its course, more speedily intense, and more diffused, because dependent on a common cause of disturbance which affects the system generally, instead of being confined to a few persons particularly predisposed to pulmonary failure. In all essentials the symptoms in the acute and extended form are the same; they start from a catarrh, and are attended, as might be expected

from their rapidity, with more or less of febrile disturbance. They are, in brief, symptoms of a *neuroparesis*, with pyrexia.

From the first the symptoms are nervous in character. The pain, the heaviness of spirit, the languor, are all characteristic of organic nervous shock. The local symptoms in the pulmonary organs are of the same type. The dulness preceding crepitation; the irregular extension of the pulmonary lesions, in patches, over the lung; the invasion of both lungs at different points; and, in some instances, the sudden congestion of the structure of both pulmonary organs—these signs all point to nervous failure as distinct from acute sthenic pneumonia, as we commonly understand that affection. The character of the expectoration also is special. The well-known rusty expectoration of ordinary pneumonia is not presented in distinctive manner, nor does the urine follow the same changes in relation to the chlorides. There is also another condition which markedly distinguishes the pneumonic paresis from the specific sthenic inflammatory pneumonia, I mean the quickness of the changes, not only from bad to worse, but from bad to recovery. I was called to a patient late one night in consequence of the danger arising from a sudden and extreme congestion of both lungs, from their bases to a point three inches above the apex of each scapula. The resistance to the circulation was extreme, and under the resistance the febrile excitement was considerable, while, from the imperfect aeration of the blood, the cerebral oxidation was vehemently disturbed. It seemed as if death were inevitable, and so it would have been if the commanding influence had continued to exert its sway; a few hours, in fact, would have been sufficient to bring life to an end. It was like a process of rapid destruction of respiratory function. But twenty-four hours later all

the general symptoms were relieved in the fullest degree, and the respiratory murmur was so clear that, if my own ear had not heard the difference, I fear I should have distrusted the evidence that might have been brought before me in regard to the modification that took place in so short a period. This was a rather extreme case, and I doubt not that during the late widespread epidemic multitudes like it in the way of rapid intensity of symptoms and comparative rapidity of relief—leaps into and out of danger from pulmonary lesion—have been observed. But this is not the feature of ordinary sthenic pneumonia. It is the feature of a sudden paresis from some temporary nervous shock and nervous failure.

There has been another peculiarity in this epidemic relating to the bronchial complication and the bronchial discharge. There has usually been some excess of bronchial secretion in advanced stages of the affection, but not of secretion of the same tenacious character, and in the same excess, as in common broncho-pneumonia. Hydrops Bronchialis has not, in my observation, been a prevailing cause of death, neither has it been, except actually *in articulo*, a troublesome symptom. The term "simple acute bronchitis" could not be applied readily to the cases of most marked character coming under the epidemic; and, when bronchial symptoms have appeared, it has been surprising to see with what rapidity they have disappeared as the influence at the root of the mischief has passed away.

The symptoms in sequence, what some have called the secondary symptoms, have borne out remarkably the idea of the nervous origin of the disease. The cerebral attacks are either dependent on the pulmonary disturbance, or are due to the same influence, interfering with the nervous governance of the cerebral circulation, as

that which interferes with the pulmonic circulation. But the cerebral lesion is more continuous, as is common to cerebral and nervous injury; hence the often prolonged stretch of nervous symptoms which follows a fairly quick recovery from the acute stage of the disease. At one time I thought that possibly there was formed in the blood, under the perverted oxidation that is in progress, a new substance of toxic character; and this, indeed, may be the fact. The carbon of the blood cannot be naturally oxidised, and therefore the nervous oxidation in the great centres will be perverted. This will, of necessity, lead to disturbed cerebral function, to delirium, to water pressure, and to the coma which is so often a prominent symptom of the later stages in fatal seizures. This central nervous failure would lead in its turn to the congestion of other vital organs, like the liver and kidneys, that are under nervous control, and depend on nervous supply for their natural activity. Thus the mischief of the neuroparesis, commencing in the pulmonic circuit, extends to the whole system; and in observing the symptoms we are practically watching development of phenomena, precisely as when we are watching the development of anæsthetic symptoms under the administration, by inhalation, of a narcotic vapour or gas, like nitrous oxide, ether, or chloroform. And, the perfection of our art should be to place the patient under such conditions that the influence causing the symptoms shall be neutralised, and the body be so circumstanced that the natural acts shall swing round into their usual course.

LESSONS FROM SELECTION OF SUFFERERS.

It has been observed in the clearest manner that the aged and feeble are those who succumb most readily to epidemic neuroparesis. This is what might be ex-

pected. The aged and infirm die almost always, when they die a natural death, in the strict sense of that term, by a natural pneumo or neuroparesis, ending in anæsthesia and euthanasia. They sink under influence of cold, fatigue, privation, fear, grief, or some other depressing cause. I have shown many times that so decisive is cold as a depressant, the fatal effects of it can be measured according to age. In the mortality produced under such depressing agency the process of failure commences in the respiration, and the comatose death which is induced is truly death by anæsthesia. Under states tending generally towards the same depression from an epidemic influence widely distributed, there can be no surprise that an intense mortality should be for a time established, in the absence of any positive mode of meeting the danger by true curative skill.

THE CAUSE OF NEUROPARESIS.

What is the cause that is at work? It must be something extremely simple, and straight before our eyes, if we could but see it. It is something that acts very much like cold and other nervous depressants. I fear those are on an entirely false scent who are looking for influenza microbes that will set up, as it is surmised, a fermentation, and produce a disease running a regular fermentative course. I think I must have been far nearer the cause in those experimental researches recorded in past labours in which I induced pneumo-paresis and the general symptoms that follow upon it, in one instance by subjecting an animal to ozone and in another instance by subjecting an animal to the diluted vapour of amyl nitrite, in conjunction with cold. In both instances extreme congestion of the lung was produced, with cerebral excitement as a succeeding symptom, and with visceral congestion, ending in death,

if the condition of the air respired were not changed in time. I do not know which of the two inhalations was most potent in producing the congestion of the lung, the ozone or the vapour of amyl nitrite; but on inhaling the two factitious atmospheres myself, at different times, for the purpose of comparison, I found the action of the ozone nearest in effect to the symptoms of what is known as influenza. In the first place, after breathing the ozonised atmosphere for a short time, the sensation of having taken a severe catarrh was always produced. There was a sense of irritation in the nostrils; there was a general feeling of chill, with nervous depression and the well-known *malaise* which is so characteristic of bad "cold in the head." This was followed by frontal headache, constriction of the breathing, soreness of the throat, and increased nervous oppression. Further than this I did not venture to follow up the investigation in the human subject; and, indeed, it was far enough, for, in one of my inquiries with my late friend Professor Thomas Wood, we both suffered from the effects induced in ourselves for three entire days. But when in lower animals—the rat and the guinea-pig —the exposure to the ozonised atmosphere was carried further, the condition of the circulation through the lung was identical with what transpires in the human subject smitten with the acute neuroparesis of so-called influenza. Congestion of the lungs commenced at the base, and extended, sometimes uniformly and sometimes in irregular patches, through the pulmonic structures; congestion of the vessels of the brain, the kidneys, and the liver followed; and, if the inhalation were continued long enough, death invariably ensued from the failure of the circulation and coagulation of the blood in the heart and great vessels. In some instances, too, death occurred some time after removal from the ozone, the systemic

effects excited being sufficient to lead to a fatal termination. It was in 1863-4 I made these observations, but the facts are as fresh before me as ever; and I recall an idea I then had, that if the atmosphere generally were, by any electric influence, to become surcharged with ozone, there would be no escape from a universal catarrh, which would in many instances run into pulmonary congestion, attended with fatal results in the aged and enfeebled of the population.

Some other facts of considerable moment came out of the investigation. It was found that, as a rule, more ozone remained in a cold than in a warm temperature. In a warm temperature, 70° to 75° Fahr. for example, almost any organic dead substances present would so seize on the ozone of an ozonised atmosphere, that the presence of the ozone could not be detected after it had passed over such substances. But if the same kind of substances were placed in contact with ozone at a temperature of 45° to 50° Fahr. the ozone remained much longer unchanged; that is to say, its combination with the organic matter was delayed. The same or similar action was demonstrated on living bodies. Animals would die in an hour if placed in an air charged with ozone at 70° Fahr.; but another animal of the same kind would live for twelve hours in the same kind of atmosphere at a temperature of 45° Fahr. Phenomena very similar have been observed in influenza, the most rapid deaths being those which have occurred in summer heat. On the whole, ozone, or a substance like it, is sufficient to account for the phenomena of disease included under the head influenza; and it has the further quality that it acts, though with varying intensity of force, in all climates. In warm climates its action would be more quickly developed than in cold, but the difference in effect pro-

duced would merely consist in difference of time as measured by hours, not days; for when once its paralysing action has become determinate under cold it is more determinately fatal. Thus in a warm climate deaths from it would be more rapid when it did strike fatally; while in a cold climate deaths would be slower in individual cases, but in larger numbers over a longer period of time.

Theoretically, ozone or an allied cause is sufficient to account for the phenomena, and, according to the Newtonian principle that no more causes of things should be admitted than such as are full and sufficient to account for the phenomena, there is, at least, *prima facie* evidence in favour of the view that an allotropic condition of the air may be the cause of acute neuroparesis.

This supposition is strengthened by certain collateral circumstances which are not undeserving of serious consideration. There are two remarkable facts which have been generally commented upon, and are truly significant. The first is the comparative immunity of children and young people from the attacks of the disease; the second is the frequent escape from any special outbreak of the affection amongst those who live in insanitary places. A true zymotic disease, a disease in which the natural zymosis is intensified, and which would run a regular course and be obviously propagated from one person to another, would attack most freely of all the young and susceptible, spreading from child to child in unmistakable line of contagion or infection, while the aged, the feeble, and better protected, would most easily escape. Also in the case of a true zymotic disease those communities would suffer most that were under greatest uncleanliness and in the worst sanitary conditions. But in the current epidemic these rules are not on the record

as the leading rules. On the contrary, the most common victims, and the victims who have shown the largest mortality, have been they who, having grown enfeebled under the protection of personal comforts required by age, mental work, or enervation of luxury, have failed, through their weakened nervous system, to meet the depression that has come across their path. The cause at work, that is to say, is one that sweeps down the people of all classes in whom the nervous powers are lowest, so that there could not be a better test of sound nervous vitality than the escape from an attack of influenza when it is prevalent in its acute form. This is entirely compatible with the action of an atmospheric influence capable of affecting the expanses of the nervous system, and through them the centres of that system in states of enfeeblement; whilst it is incompatible with anything like a sthenic inflammatory condition, depending for its nurture on a strong and vigorous organism capable of supplying the pabulum for the disease and of throwing off from itself the poisonous particle that, within itself, has been developed. Lastly, as to probabilities, there is nothing but what is quite reasonable in the hypothesis of a chemical atmospheric origin of neuroparesis in an endemic or epidemic form. Dr. Chisholm observed in New Granada a distinctive endemic outbreak in which death was rapidly induced in a number of negroes, in consequence of the separation of fibrine in their blood during life and its coagulation in the right side of the heart; and he traced the cause of the affection to the exposure of the victims to a peculiar dry and heated air driving down through a ravine upon the plantation on which they were at work. Sydenham held the most common-sense views on what he called the "epidemic influence"; and we are all conscious of the effects of

different electrical conditions of the air to which the nervous amongst us are so vividly susceptible. It would require, indeed, very little modification in the composition of our atmosphere to render all life upon it a vast theatre of disease and death. A sufficient electrical disturbance to make a part of the oxygen of the air unite chemically with the nitrogen, and the new product, mixed by the winds, so as to enter the lungs of the breathing community, would send the whole of us, big and little, strong and weak, into fits of uncontrollable laughter, into dreams of wildest rapture, into sleep as profound as that which allows us to have a tooth extracted without the merest realisation of suffering, with lungs congested, and all the other visceral organs so full of blood that if we were not speedily relieved from the lethal condition we should simply be left to sleep to death. If these events could happen from changes of a major kind in the atmospheric sea surrounding us—and the marvel is that they do not—shall we be surprised at minor events from the effects of minor changes in the same sea? In the course of the ages what matter in space does not our mother earth pass through in her course around the sun? When she, with her atmosphere, comes into contact with solid bodies attracted by herself from space, they become luminous and give us meteors. We appreciate then visible changes progressing in the air, each one of which must produce, in its limited sphere, some degree of chemical disturbance in the atmosphere. But what we see is very small to what must be constantly going on out of our sight, and that the ordinary relationships between the two elements oxygen and nitrogen should be perverted is the most likely of all cosmic phenomena. There seems, in fact, to be no reason for stability between the two uncombined atmospheric elements, for we know that, from some unknown cause,

oxygen is presented as ozone, which means that oxygen has been condensed in air; and we also know that the liberation of the condensed gas may take place in oxidisable matter without seeming to disturb the relationships of the remaining oxygen to the nitrogen. In addition there are certain telluric influences and certain winds, perhaps certain sun storms, which may lead to electrical, or chemical, or chemico-meteoric changes in our atmosphere all-sufficient to bring the nervous animal function into feebleness—neuroparesis.

I have put forward now some of the arguments which tell in favour of one widespread cause of neuroparesis. Let me turn next to the arguments that may fairly be brought against this supposition. These arguments are two in number. They are very distinctive in character, and they are confessedly powerful, though, as I shall try to show, not unanswerable.

The first argument is that the disease called influenza is contagious, that it passes from person to person, and therefore must be produced by something which is conveyed from one person to another. But what passes from one person to another is something, it is assumed, that has been cultivated or secreted by the affected person, and cast off from him, to be transmitted to others, to be further propagated and transmitted by them, in a continuous line of contact. Granting this argument as true, there is no necessity, it is urged, for the existence of an all-prevailing influence; the seed of the disease is disseminated, is borne from one to another, is multiplied in each person attacked, and so, like a multiplying seed, grows and produces in the living bed that sustains it all those departures of physiological phenomena into pathological which give the form and the expression of the malady. On this hypothesis it is contended that

although season and peculiar atmospheric states may favour the development of the organic particle that is the actual cause of the disease, the particle itself is specific, and is the cause. It may be a microbe, it may be a particle of a modified secretion from the affected body, but it is the movable, transmissible exciting cause. Received into the susceptible body, this particle, either by its own multiplication or by a secretion which it produces, sets up a change, under which the morbid phenomena characterising the disease are induced. It matters not for the sake of the argument what the phenomena are, nor whether their seat be in the blood, in the membranes, in the nervous system: the cause is one and the same.

At first sight it looks as if there were no answer to this assumption. It appears to carry all before it, and for the moment it satisfies multitudes by the appearance of its simplicity. It is, however, surrounded with improbabilities when it is carefully investigated.

To begin, the fact is obvious that in the presence of a diffused and external influence everywhere present the idea of a contagious influence must of necessity be suggested, though the absence of contagion were as far from the truth as it possibly could be. If a number of men were wandering in the air of a mine, an air not poisonous enough to kill at once, but sufficiently poisonous to produce symptoms of disease, nothing would be more likely than for the last man affected to believe that he was affected from approaching or helping some other man who had previously become affected. If he did not know that an atmospheric poison were present, how could he come to any other conclusion? And, if those observing were as ignorant as himself of a true cause, how could they come to any other conclusion? It would be the most likely thing in the world that every attack during or following close

proximity to some person attacked would be attributed to proximity; and, on the method of mere general observation, it is hard to see how the idea of transmission by contagion could be avoided.

But in the case of true contagion there are certain elements which are essential for its establishment, and which cannot be absent. In true contagious epidemics there is less distinction of persons. The young are stricken as well as the old, and, as a general fact, the child population suffers most acutely. It is not the aged and enfeebled that are selected, but all who are exposed. In influenza epidemics the rule thus indicated is not in any sense or marked degree presented. In a true contagious epidemic the sufferer who has once contracted the disease is protected from its recurrence, for some years at least, except in very rare instances; in influenza there is no such rule of exemption: the disease may recur within a few months, or even weeks, in the same person. In true contagious epidemics there is a decisive period of incubation, varying from two to four, six to eight, fourteen to eighteen, or even twenty days, according to the disease; in influenza there is no such definite period, and in truth no marked incubation at all. In true contagion there is a regular course of the symptoms without any such circumstance as a sudden relief of acute symptoms and an almost instantaneous recovery; in influenza these phenomena are not unfrequently presented. In true contagion the clothing and other effects that have been in contact with the sick are capable of carrying the disease; in influenza I have never seen during the whole course of three epidemics, including that of 1847, anything that could lead to a suspicion of such conveyance of the affection. These differences, alone, appear to my mind to cast the most serious doubts on the whole theory of contagion,

a theory which in 1847 was never more seriously argued, than in the same way as people are accustomed to say that a common cold is catching, and therefore runs through a household.

Here is the mode by which the theory of contagion is supported. A lady suffering from influenza comes from a distant part of the country into a house where many persons are living, and in a few days the greater part of the inmates of that house are smitten with the same symptoms in a greater or lesser degree. Such an outbreak occurred under my own knowledge. How plain it looked. But correct that observation by another. At a dwelling within easy distance of the same house one person was attacked most severely with the disease without having the remotest suspicion how he contracted it, and for several days he lay in the extremest danger. His house was well filled with people residing in it, there was no quarantine whatever, and there was no hesitation on the part of any one to attend in the sick-room. The house was not specially sanitary, and the sick-room was almost in the centre of the house. Yet not a single person in the residence had the slightest touch of the affection. This last example might be multiplied to any extent; but it is so peculiar as opposed to the theory of contagion, that one illustration alone is sufficient to throw the most serious doubts on the progress of the malady through contagion.

Another doubt is introduced by the apparent spontaneity of first cases. A number of persons are seized at once; sometimes they recover as mysteriously as they are stricken, they having never been near to any centres from which to receive infection. One of my friends told me of a vessel that sailed to an island with a number of sailors stricken with influenza. "At once," said he,

"some people of the island became attacked." "But where," I asked, "did the affected vessel pick up the disease?" That was the strangest matter of all; the vessel was several hundred miles from land when the crew was attacked, and had not been near a possible source of contagion. The same kind of observation is made on land. I remember in the epidemic of 1847 that one day there was no such disease known as influenza in the neighbourhood of the little market town in Essex in which I was then engaged in assisting a practitioner who had a large and widespread practice. The next day there was an outbreak which almost paralysed us by excess of labour. I was in the saddle riding from one village to another for twelve hours, doing over sixty miles in that time. The cases were at all points of the compass, and the idea of any line of contagion was simply absurd. In some instances all the residents of a house were stricken at once; in other instances one alone was stricken; and, again, one would be stricken, and others would follow. The children escaped; the aged and infirm suffered most. I never recall an example in which so many fell ill all at once, but the treatment was simple, and the mortality not great. It was, in short, the stroke of a general influence which did not travel in horizontal lines over the surface of the earth, but descended upon it like atmospheric pressure; and this has been the order of events in succeeding outbreaks.

Taking them all, the arguments that have been adduced in order to bring the phenomena within the lines of the germ hypothesis of disease are singularly strained. The remarkably small microbe, which is assumed to be the cause of the influenza plague, must be a living thing capable of defying all powers of death. No particle of dead dust was ever more indestructible. It is as gene-

rative and lively for mischief in regions of thick-ribbed ice as it is in the heat of a tropical sun. It chooses the gilded chambers of the great for its field of action in preference to the miserable hovels of the poor, the west of a great metropolis like London in preference to the east. It sets sanitation at defiance, and as if to show its independency, in this late epidemic, it settled down upon the medical officers of the Government themselves as if it wished to say to them, "It is your business to study me and know my doings; here I am in your very bodies; here I am with you night and day; study me, but master me never!" It has a dislike for young children and a special attraction for old people, on whom it seizes and whom it lays prostrate. It comes like a thief in the night, and retreats in the same manner. Sometimes out of a family it infests one member and avoids the rest; sometimes it seizes on every member, or on the greater number, letting one or two escape. We ask where it comes from. There is no answer. We ask where it goes to. There is no answer. When a person has been smitten by it we ask:—Has he been smitten by a single microbe which has developed in his body, or has he been smitten by a swarm of microbes as he might have been by a swarm of bees? There is no answer. We ask, again:—How was the microbe taken into the body of the person affected by it? Was it taken in by the lungs; was it absorbed by the pores of the skin; was it swallowed by the mouth? There is no answer. Strangest fact of all in regard to this mystical microbe, although it can live in any climate, and although it has planted itself, on the largest scale, in some favourable locality, instead of remaining there and depopulating the place, it suddenly ceases its ravages and for a season disappears.

These are the difficulties attaching themselves to a

revived hypothesis now as generally accepted, as if it were gospel truth. All I can say respecting it is, that if influenza be dependent on a germ or microbe, then every common cold a person takes is dependent on the same or a similar cause, for in every particular the taking of a common cold and the taking of influenza are identical except in matter of degree. In both a disturbance in the nervous system is the primary cause of the symptoms, and in both the course which the symptoms take depends on the impression of the first nervous shock. But those who are very susceptible to colds know perfectly well that they can give themselves a cold at any moment they choose. A sudden change of garments will suffice to produce the affection; sitting in a draught of air passing through a chink or a keyhole will do it; getting wet in the feet will do it; sudden change of air will do it; whilst anything that reduces the nervous tone will intensify the danger of each or any of these exposures. In childhood the susceptibility to colds is infinitely less than it is in advanced age, and is also less in men who are inured to cold and wind, like drivers of railway engines. On the other hand, those who, being accustomed to live in close, warm air, are suddenly exposed to damp or cold air; those who are always changing from shelter to exposure, like railway porters; those who have to walk about in the cold and damp and are subjected to considerable muscular fatigue, like policemen and postmen—those all are liable to colds, and in similar manner are liable to influenza. Influenza, in a word, is, as the older physicians said it was, "an epidemic catarrh." If a catarrh is infectious or contagious, influenza is, and the evidence in favour of infectiousness or contagiousness is the same in both affections. A cold or catarrh runs sometimes through a house. Is that evidence of contagion? Influenza, or

acute epidemic neuroparesis, stands in the same doubtful position.

As we reflect on the phenomena of catarrh and influenza, there come forcibly before us two elementary questions which through all the long ages of physic have never yet been solved, although the facts that give rise to them have so long been under review.

The first of these questions is: What is a catarrh? It is a nervous disorder, a nervous shock or depression, a neuroparesis followed by pyrexia. But what is the reason of it? A man takes or catches cold. Why? He passes through a certain well-known series of changes from natural function and then recovers. Why? If we could solve this one primary and simple question influenza would be no mystery. Colds are the origins of three-fourths of our acute diseases; they are constantly under our observation; they attack individually; they attack endemically; they attack epidemically; they leave different trains of disease; they specially afflict some persons; they let others go free. Why? It is mystery. All we can say is that a cold is the effect of some untoward impression carried from the peripheral nervous expanses to the centres, and that the further phenomena are reflexes from the centres. We are accustomed to assume that the respiratory mucous surface is the surface first attacked or impressed, and often it is so. But the rule is not universal; if it were why do wet feet cause the primary phenomena? Why does exposing one-half of the body to colder air than the other half do the same thing? Why does exposure of the loins to extreme cold do the same, with special tendency to reflex rheumatic symptoms?

The second question of moment relates to the physiology of contagion. To believe heartily in what may be called a specific blood or fermentative contagion, a contagion

in which, as in small-pox, some material thing is carried from one living body to another living body, and multiplies by contact diffusion, is at present the humoral and accepted doctrine. But is this altogether correct? Is there not such a thing as nervous contagion, contagion of sympathy, contagion through the senses and the common sensibility, which, though infinitely more subtle than humoral contagion, is far more pervading and infinitely more active? I have myself no doubt on the matter, and I strongly suspect that the humoral contagions themselves may have their original start from such nervous source. In this sense catarrh and influenza, and neuropareses generally, with their sequælæ, may become contagious through susceptible natures, just as in panic when one bearer of terrible news, stricken with fear, infects another or others with his symptoms until a veritable endemic or even epidemic of fear and faintness is the communicated and communicating catastrophe, before which persons of the weakest nervous organisation, the nervous and jaded, are the first to succumb.

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."
BENJAMIN RUSH.

DRY AND MOIST FOGS.

THE symptoms complained of from fog differ much according to individual constitution and age. During the recent fogs in London I have inquired very carefully into the matter, and have tried to discover if there are any particular symptoms which admit of being grouped together under distinct forms of fog. To some extent it may, I think, be admitted that groupings are possible; but the distinctions are not peculiarly clear, if the term "fog" be not itself in each case defined. The character of the fog makes a decided difference.

Some fogs are moist; some are dry. On the day when I am writing this article—January 24th, 1892—we have in the part of London where I reside (Manchester Square) a dense fog; but although I am unable to move from one room to another without the aid of artificial light, the hygrometer in my library records only 38 degrees of moisture in the air, and the barometer is at $30\frac{2}{10}$. This therefore may be called a dry fog; it is unpleasant, and it must be unhealthy, but it is not particularly chilling. The symptoms arising from such a fog are those of nervous irritability without feverishness and without unusual depression. I do not notice

that they who are sick are very much injured by the fog, nor are they peculiarly depressed by it; those who have bronchial disease are most affected locally, and they not so much as might be expected. The healthy complain variously. One says the fog renders the eyes irritable and causes a free secretion of fluid, with constriction about the forehead and a sense of weight in the head, but without actual catarrh. Another complains of constriction and dryness of the mucous membrane of the fauces, with an extension of the same along the bronchial tract, and with a certain amount of constriction of the chest. A third is conscious only of muscular weakness. A strong middle-aged man says he feels "as if he were drawn by a magnet whenever he seats himself in a chair"; the chair, that is to say, seems to draw him to itself; and when he lies down in bed he feels as if all his body were drawn earthwards in a similar manner. In plain language, the fog makes him physically feeble. On the other side I have found some persons who, although advanced in life, are in excellent condition, and say they feel in good spirits, but regret they cannot, because of the darkness, get out of doors for exercise with their usual facility. I notice now also, as I have noticed in previous conditions of a similar character, that no great excess of catarrh, rheumatism, or neuralgia prevails. One, all but confirmed, rheumatic sufferer, under my frequent observation, is at this moment freer from pain and more active than she has been for many years past.

Those who have chronic bronchitis or asthma during dry fog with a high barometer suffer from irritation in the bronchial passages, and expectorate a secretion which for colour almost approaches soot-black. They

do not, however, complain seriously, and one asthmatic I know is relieved considerably. Altogether the dry fog is a troublesome, but, barring physical accidents incident to the darkness it causes, it is not a severely dangerous enemy.

The moist fog is quite a different enemy. It is directly bad and dangerously bad, whether it occur in warm or mild or in cold weather. It is a cause of chill, of catarrh, of bronchial mischief, of rheumatism, of dyspepsia, and of depression of mind and body. It intensifies the symptoms of all the acute diseases, and it aggravates those of a chronic character. The primary evil of the damp fog is that it suppresses the function of the skin, and prevents the free elimination of water from that eliminating surface. Under this the balance between the venous and arterial bloods is too equal, and the venous blood fails to exercise its full power of absorption from the different organic structures. There is an approach to osmotic stasis which must at all times be a dangerous condition, a condition at best incompatible with healthy life. Under such condition there is an imperfect oxidation, an accumulation of dead material within the body, which—to use a commonplace simile—like dust in the works of a watch or a clock, makes action slow and laborious. In the dry fog evaporation may continue unchanged; in the moist it never can. Again, in the moist fog the action of the water vapour, in which the body is enveloped, is rendered more perilous by the fact that the water carries away, with great rapidity, the vital heat. The clothing around the body becomes charged with moist vapour; the surface of the body is in contact with the moisture of the clothing; the moisture of the clothing is in contact with the moisture of the air, and every wind or draught that

blows carries off the radiating heat and leaves the body cold. In that kind of air surcharged with moisture, as well as dust, the statical electrical machine will not act in the free and ready manner in which it does in the dry fog. The living body is in the same predicament.

So it turns out, under observation, that the state of the bodies of men tells of itself the nature of a fog, without the necessity of referring to hygrometers or other physical instruments; in fact, the body of man is itself a test instrument. In the moist fog there are experienced all the states of an impeded action. A cold passes readily into bronchitis or pneumonia, acute or chronic rheumatism, congestion of the liver, a disturbance of the stomach or bowels, or a nervous outbreak of pain, as well as into depressions physical or mental, or physical and mental.

These facts have an immediate and all-important bearing on practice. Hitherto the word "fog" has too easily been accepted as indicative of one state of the outer air alone, and therefore indicative of the same dangers under all circumstances. Darkness only has been accepted as the sign of evil. This is not correct. The proper exposition would be more nearly rendered by saying that a dry fog is inconvenient, a moist one dangerous. When I enter my consulting room in the morning to find it besmirched with fog, with the windows darkened by the cloud outside, I turn to the hygrometer and the electric machine near by. If I find the air free of excess of moisture, and if a few turns of the electric machine give me good sparks, with ozone, I know that, whatever the temperature may be, the fog is a dry fog. But if the moisture of the air be great, and the electric spark all but impossible to obtain, I know it is a moist fog, and on those distinctions my recommendations

largely turn for the day. Against the dangers of the moist fog I warn every patient; I request all to keep in a warm atmosphere, rendered as dry as it can be made; and in particular cases I instruct how to filter the air and dry it specially as it enters sick-rooms; I forbid outdoor exposure, and I take great care in directions as to clothing and regimen. When the fog is dry, I omit many of these precautionary measures, and, except advising that a porous comforter should be worn over the mouth and nose in order to filter the atmosphere, I enforce none of the stringent rules, nor any rules stringently, as in the case of the moist fog. Also in regard to travelling I make this marked difference: that in the moist fog no risks must be accepted by the sick, whilst in the dry fog, with moderate precautions, travelling may be carried out with comparative safety.

I notice, lastly, as between the effects of dry and moist fogs, that pain is a marked and distinctive test. Rheumatic pains, neuralgic pains, and toothaches are so specific to moist fogs, they will, of themselves, differentiate one fog from another.

DEFAULT OF CILIARY ACTION AND DISEASE.

THE default of ciliary action in the respiratory tract must have a most important bearing on disease, especially during the presence of fogs, whether they be moist or dry. Strong men with large powerfully acting chests, who move about in fog and dust, suffer considerably from loose cough, but are not long troubled by it. They cough up large quantities of secretion, which is as black as if soot had been mixed with it, and which is, in fact,

bronchial mucus loaded with soot. The fine particles of carbon in the air have been carried into the minute bronchial ramifications, and have been brought back into the larger bronchi from tubes, in which they would easily produce dangerous obstruction but for the active motion of the ciliary fibres. This motion is going on steadily night and day, and is relieved by frequent cough, through which small quantities of mucus are expelled. During the night when the cough is quiet the mucus carried to the upper portion of the bronchial tract accumulates there, and gives rise, on awakening, to a series of coughs, during which free quantities of dark mucus are expectorated, and all accumulation is thrown off. It is a good sign of healthy lungs when a free expectoration of dark sputum is followed by a small quantity of white clear mucus. The difference indicates that the ciliary vitality is sound, and that the lung remains, on each side, a good filter.

As a general fact the ciliary vitality runs with age: in the young it is most active; in the middle-aged it continues good; in old age it begins to decline; and in the very aged it probably ceases altogether. In the chronic bronchitis of the aged the absence of ciliary motion must add considerably to the danger of inhaling atmospheres charged with particles of smoke and dust. The knowledge of this very simple and elementary physiological fact is full of meaning of a practical kind. People as they get on in life are apt to become of indolent habit. They like the luxury of the easy-chair and chimney corner; they dislike to face the weather. The old scholar gets into his library, and there in the midst of his books, which are the worst accumulators of dust in the world, he luxuriates in a very ocean of fine particles which he does not recognise.

If a ray of sunlight strikes across his room he is startled at seeing in what a cloud he is immersed, or if he takes down a few books all at once for a search, he is surprised to observe how he has blackened his hands, and what a quantity of black sooty matter he coughs up afterwards. The old lady in her snug drawing-room, with its cushions and stuffed chairs and tasty bric-a-brac, sinks into similar luxurious ease, and observes when she goes in for putting things a little right the same signs of dirty air. These signs are good guides for the management of old bronchial passages, with their lifeless cilia. They tell the aged that when they are obliged to stay within doors in dull and treacherous seasons the air inside may be as injurious as the air without. They declare as clearly as if they were speaking phenomena that the air of the apartment is dangerously full of dust, and that the ciliary brushes are not in sufficient force to clear the passages lying between the air and the blood. Hence the moral. Have the air of the room cleansed, and, if it be possible, go out into fresh air to give the lungs a good chance of purification. In my practice I am often consulted by men of business, or of letters, who, having passed the meridian of life, still pursue, and would very judiciously pursue, their usual avocations, except that in their offices or libraries the air is bad from the presence of dust. To these, suffering from bronchial cough with dark sputum, I always prescribe a day or two of country air as the lung-purifier of the best sort. In mild, warm weather the sea air may be the best for such persons, but in less promising weather country air, in a pure locality, is just as good. Nor is it necessary to go far from a town. To the Londoner the air of Sydenham or

Chislehurst is excellent; and that of Hampstead Heath is good beyond dispute.

THE BRANDY FETISH.

IT is well, as time wears on, and as the superstitions of past and ignorant ages wear out, to correct, not only actual practices which are hurtful, but the terms also by which bad practices are designated, and on which superstitions sometimes hang when real falsity has been discovered and proclaimed. I venture, therefore, the suggestion to change, in medical practice at all events, an old and misleading term, *Aqua Vitæ* or *Eau de Vie*. Why brandy should ever have been called, by any persons, *water of life* instead of *water of death*, is very peculiar, considering its manifold, and obviously manifold, faults, and the fearful mischiefs it has inflicted on mankind ever since it was first imposed on the world. But I am dealing with it now simply as a medicinal substance. It seems to me, we men of physic have, like the rest of the world, got it so firmly implanted in our minds that this fluid is really a kind of water of life, that we order it as such without a thought of questioning its value. There was confessedly a time in my life when this was my folly, and I fear there are some who still adhere to the same idolatry. Not long since I was called to meet a brother practitioner in a case of typhoid in a comparatively poor neighbourhood. Being a minute or two before my time, I observed a woman enter the house of the patient as one who belonged to it, carrying under her apron a bottle, which she took into a parlour on the entrance passage floor, leaving the street door of the house open, and coming back to it to meet my

friend, whom she had seen approaching. The case was a clear case enough of typhoid, not of a severe type, but peculiar, in that there existed, with a great deal of mental stupor, a constant subsultus which was easily intensified by touching the skin over various groups of muscles. There was also an ethereal odour in the breath, with a temperature much lower than would be expected; that is to say, half a degree of fever only. The practitioner in charge pointed out these peculiarities, which he said he could not account for. After we retired for consultation in the parlour in which the bottle still stood, I took the liberty of uncorking the bottle and of smelling and tasting its contents. It was as vile a specimen of an article called brandy as could be met with. When a little of it was burned, and a white plate was held over the flame, a dark deposit of carbon showed the presence in it of amyl alcohol and explained at once the cause of the peculiar delirium and subsultus. The man who was taking this brandy was suffering from the action of amyl alcohol, mixed with the ethylic alcohol in the brandy. My friend was willing to admit this view, but, he argued, had not the reduced temperature which had come on since he ordered brandy produced an effect? It may have done so, for no one of the members of the alcohol group reduces temperature like amylic alcohol; but this was not the important point. The point was that amylic alcohol had been brought into use under the name of brandy, and was producing its own specific effects without the recognition of those effects in a precise and scientific manner. The observation might fairly lead to an inquiry as to the value of amyl alcohol in typhoid, but it said nothing for brandy as the name of a remedy. In this instance we at once

stopped the brandy, with quick subsidence of the subsultus, with return of the mental faculties, with clearance of the breath from the ethereal odour, and with an increase, for a few days, of fever up to two degrees, but with excellent recovery without the aid of an alcoholic of any kind.

More recently a child fifteen months old, coming from abroad, was brought home after suffering on board ship from an acute exanthem. The child was feeble and disinclined for food, and had a white creamy tongue, with several small, very irritable aphthous spots upon it. Under medical advice, the mother of this child was led to administer to it brandy half diluted with water in small doses, and to trust to that as the "sheet anchor." How the poor infant could swallow such a fluid was a marvel; and that it should try to resist taking it was, indeed, no wonder. Under the so-called remedy, it fell into a semi-comatose state, and in this condition was seen by an able practitioner, who was led at first to the diagnosis of subacute meningitis. When, however, he had gleaned all the facts, his eyes were opened; he stopped the brandy, and on substituting for it plain nutritious diet and simple remedies, the little patient gradually came out of the coma and made a rapid recovery. Here was a case in which the word "brandy" was the misleading mischief. On what hypothesis could brandy have been prescribed under the circumstance named? How could it take the place of food? What was it intended to do? Besides, no instruction was given as to the character of the brandy that was to be used. Suppose we ordered other drugs of potent toxic action in the same loose way, what would happen except frequent deaths, with an occasional inquest, followed by a trial for malapraxis? It is time to give *aqua vitæ* its true name of *aqua*

mortis, and to let the fetish, brandy, stand for a fluid the chemical qualities of which are uncertain and the physiological utterly unreliable.

INDIAN HEMP AS AN INTOXICANT.

INDIAN hemp is probably older than wine as an intoxicant. It is the *Nepenthes* of the poet Homer. The father of history, Herodotus, also refers to it in his first book. He tells us, in writing of the people living on some small islands on the river Araxes, which runs into the Caspian Sea, how they discovered plants that produced a fruit which, when they met together in company and had lighted a fire, they threw on the fire as they sat round in a circle, and by inhaling the fumes of the burning fruit that had been thrown on the fire became intoxicated by the odour, just as the Greeks did by wine; and the more fruit that was thrown on, the more intoxicated they became, until they rose up to dance, and betook themselves to singing.

To this day the *Nepenthes* of Homer, known now in some countries as Haschish, is employed as an intoxicating substance. Like opium, it is fitted either for a pill or a pipe, and, according to taste or circumstance, is used either as a medicine or a luxury.

Whatever the form of its preparation, its active principle is derived from the flowering extremities of the variety of hemp called Cannabis Indica. The active substance itself is fixed in a resinous principle, which is called Cannabina or Haschishina. The resin is not met with in hemp that is indigenous to cold or temperate climates.

Haschish, as it is prepared for use, does not represent

the pure resin. It is in cylindrical or candle-shaped pieces, pointed, and of a brown or almost black colour. The material, when powdered, has the character of a dried extract. It softens under the influence of warmth, and may be moulded by the finger; to the smell it conveys the idea of hemp or impure wax; to the taste it is sharp; it is easily soluble in water, and more soluble in ether; digested in ether, the impure substance yields a dark resin possessing all the properties of Cannabina.

Indian hemp is taken by those who indulge in it in various ways, according to custom or fashion. By some it is smoked in the pipe, although, like opium, it smokes with difficulty, requiring the constant reapplication of the torch; it is smoked sometimes with tobacco; it is often taken by the mouth, and may be mixed with honey or swallowed in coffee. There is also a fatty and aromatic substance prepared from it for those who choose to chew it. It is not unfrequently smoked in the crude form, as the simple flower and seed of the Cannabis Indica, or is swallowed without any further preparation than that of making an infusion of the seeds and flowers of the plant as we make tea. Occasionally it is taken in the form of a spirituous liquid, made from the plant Cannabis Indica while in its fresh state. In England those who indulge in it commonly take it as a tincture, and call it "Bang," or "Bhang."

The Effects of Cannabina.

The effects of the narcotic substance taken into the body vary according to the dose, and according to the habit of the person who takes it. According to Professor Rech, of Montpellier, thirty grains of the extract are equal in effect to one to two grains of Cannabina, the active substance. At first those who indulge in it

take from seven to eight grains or a smaller dose, but confirmed *habitués* get up even to twenty-five or thirty grains, or more. All who take it for a time become habituated to it, and get a craving for it which is very decided. I had once a gentleman under my care who took it for many years, alternating its use with alcohol, and I have had a few persons under observation here who have taken it as an extract or have smoked it. The effects produced, as described by them, are pretty uniform, and are symptoms of a peculiar intoxication. The best account of the action of Indian hemp was supplied to me in the year 1870 by my late distinguished friend Professor Polli, of Milan. This brilliant experimentalist and physician determined to make a series of experiments on himself and on two other learned friends, Dr. Vincenzo Rosa and Signor Emilio Sinistri. The specimens of Haschish used were brought by Rosa from Damascus. The three commenced their researches by each one taking seven grains of the extract; half an hour afterwards, there being no perceptible result, a second dose was taken, and a few minutes later a third dose was smoked from a pipe with some Hungarian tobacco. They were near the end of their pipes when one began to jest obstinately about some French words, and then commenced to rattle a spoon in a coffee cup in a way denoting intoxication. In about an hour and a half from the reception of the first dose the three were inebriated, the symptoms of inebriation developing themselves at last with great rapidity. Their first sensations were intense astonishment at finding themselves no longer masters of their own acts, while they still remained lucid witnesses of all their acts, however foolish, a condition which marks the most decided difference between alcoholic inebriation and inebria-

tion from the hemp. They saw themselves committing absurdities of the most grotesque kind—leaping, beating time to nothing, moving their arms as if receiving electrical shocks, writing ridiculous words, and feeling as if they were independent of the acts; that is to say, as if other persons were doing them. They had also an irresistible propensity of feigning exaltations which they did not feel. Altogether the sensations were most singular. "To feel," says Polli, "one's own identity changed, or it were better to say divided; to feel one part preserving its integrity while the other plays the fool; to find one's self strongly disapproving of the acts of a neighbour under the same influence and, while acutely perceiving the folly of his proceedings, to be unable to refrain from entering into his whim, is a mental phase full of the most curious interest."

Dual Personal Identity.

The sense of the division of identity seems, according to these observers, to be the effect of two successive and alternate states of mind: at one moment the intellect is obscure and loses itself in forgetfulness of the past; then it returns clear, but only to be again involved in automatic folly. During the intervals of confusion or darkness, lucid moments occur in which the powers of comprehension are truly marvellous, so that in a few seconds the most distinctive and accurate picture of a range of life including as many as forty years may be recast and surveyed. The alternation from obscurity to lucidity is like the effect of a sea wave. A lucid wave is followed by a dark overhanging wave, on which the mind is shipwrecked and carried, with melancholy, towards forgetfulness and oblivion, to be roused instantly by the passage once more of the wave of light

and life. The dark waves chase each other so long as they continue, and the mind being unable to command its thoughts and acts, and bending under a series of successive impressions, the shortest space of time seems to present the duration of an eternity. Another singular condition excited by this intoxicant is a moral phenomenon, namely, a common docility and absence of susceptibility which is most remarkable. "In these phenomena," says Dr. Polli, "the secret of the magical influence which the old man of the mountain is said to have had over his adepts, to whom he drank in Haschish, was fully and personally explained to us."

The effects of the intoxication lasted a long time; in one of the experimentalists thirty-six hours were required for complete recovery.

From these observations, and from others which I could supply if time and space permitted, it will be seen that the effect of Indian hemp is to produce an intense intoxication, which, differing considerably from alcoholic intoxication, is as decisive in its action and as captivating in its tendency. What may be its permanent influence amongst those who indulge in its use has not been determined as yet with sufficient accuracy to enable one to speak with the same precision as can be spoken on the effects of alcohol. We have, however, sufficient evidence of *bad* effect to be certain that the peculiar intoxication induced by the narcotic is certainly destructive to sound mental life, and that those who are exercising influence over their fellow-men of our Eastern possessions, so as to bring to an end a pernicious habit, which seems to be gaining ground there, are rendering the best of services to mankind in their day and generation.

ALCOHOL AS ANTIDOTAL TO CHLOROFORM.

IN one of the future lectures on the cause and prevention of death from chloroform, I shall have to refer to the value of alcohol in the administration of chloroform. Considering, however, the frequency of death from chloroform, it would be improper to wait until then before supplying what has been for many years, in my hands, a most useful practice. The fact that the danger from chloroform is largely owing to the early effect of it in causing spasm of the vessels of the minute circulation, coupled with the further fact that the first effect of alcohol is to cause relaxation of the minute vessels, induced me originally to experiment on the action of chloroform upon warm-blooded animals while they were in different stages of alcoholic inebriation. The result was that it was found possible to narcotise with chloroform completely without the manifestation of a rigid or convulsive stage whenever the animal was prepared for inhalation by the influence of alcohol. Thence I was led to make it a rule always to administer to the human subject a sufficient dose of alcohol, by the mouth, before administering either chloroform or methylene, and I am quite satisfied as to the soundness of the proceeding.

To carry the practice out properly, it must be done *secundum artem*. The mode of administration must be considered, and the state of the patient. The administration of the alcohol must take place some minutes before the chloroform is administered, so that time be afforded for the development of the first stage of alcoholic inebriation, as indicated by quickening of the pulse, redness of the cutaneous surface in vascular areas, like the cheeks, increase of temperature, and slight

mental excitement. To produce this first effective alcoholic stage I began—many years ago—by using sherry for the purpose; but as I got to understand more precisely the physiological action of alcohol, I gave up this utterly unreliable fluid, and employed alcohol itself, in definite dose, in combination with a small amount of chloroform. As a specimen form the following answers well:—

℞. Tinct. Chloroformi, ʒiv.; Spirit. Tenuoris, ad ʒiv.
Fiat Mistura, ʒiv.

I keep this compound ready for use in a four-ounce bottle, and find that one ounce of it agreeably diluted with water—to which a little sugar can be added if desired—is, as a rule, sufficient for an adult who is not, by habit, an inebriate. To an adult who is habituated to alcohol I increase the dose to an ounce and a half or two ounces; and in all cases, if after fifteen to twenty minutes the first stage of alcoholic action is not developed, I repeat the dose to secure its development. In so far as I can judge, this administration of alcohol should properly precede chloroform in every instance; but in all instances where the patient is alarmed and nervous it should be carried out as a rule to which there should be no exception, except inebriety be already present, when it may be dispensed with. This is one of the truly legitimate uses of alcohol for medicinal administration; and it prepares the patient for effective action of chloroform in a manner that has only to be seen a few times to be duly appreciated. Death from the first few inhalations of chloroform vapour might not perhaps be absolutely impossible in a person under the first stage of alcoholic inebriation, but it would be extremely rare.

THOMAS WILLIS, M.D., F.R.S.

WOOD, the quaintest biographer one can read in these days, introduces us to Dr. Thomas Willis in the following terms:—

"He was a plain man, a man of little carriage, little discourse, complacence, or society; yet for his deep insight, happy researches in natural and experimental philosophy, anatomy, and chemistry, for his wonderful success and repute in his practice, the natural smoothness, pure elegance, delightful unaffected neatness of Latin style, none scarce hath equalled, much less outdone him, how great soever. When at any time he is mentioned by authors, as he is very often, it is done in words expressing their highest esteem of his great worth and excellency, and he is placed still as first in rank among physicians. And, further also, he hath laid a lasting foundation of a body of physic, chiefly on hypotheses of his own framing."

The illustrious scholar in medicine, thus introduced to us, came into this world at Great Bedwin, in Wiltshire, on January 22nd, 1621. His father was Mr. Thomas Willis, of that place, and his mother, Rachel Willis, was daughter of Mr. Howell. The family appears to have been well connected, and our scholar, who was the eldest son of his parents, was sent in due time to a grammar-school under the mastership of Mr. Edward Sylvester, an

eminent teacher in the parish of All Saints', Oxford. He acquired quickly a reputation for his advancement in learning, and in his sixteenth year, through the patronage of Mr. Thomas Iles, obtained a canonry in Christchurch and the membership of that famous college. Unremitting in his studies, he progressed rapidly, and on June 19th, 1689, graduated B.A., and on June 18th, 1642, M.A.

At this time the city of Oxford was the seat of the King's government. The loss of the battle of Edgehill had forced the unfortunate Charles I. to retire to the university city and to garrison himself there. Here the King held his Court, and here, as Harveian scholars will remember, Harvey dwelt by the side of his royal master as the favourite physician. Willis, at the time, had not entered physic, but, like many other students of the university, took up arms for the King. One may imagine, however, that he came into contact with the discoverer of the circulation of the blood; and, as will be seen when his works come under notice, he was deeply imbued with the knowledge of that discovery, and with the practical lessons that were likely to spring from it. These might have been the reasons why he began, even during his military career, to devote his spare time to the study of physic. He did so, and on December 18th, 1646, he graduated M.B. In 1646 Oxford surrendered to the Parliamentary forces; Harvey left the service of the King to return to London, and Willis settled himself down in Oxford as a practitioner in St. John Baptist's parish. His house was situated opposite Merton College, and it is said of him that he "kept Abingdon Market," by which is meant, I presume, that, according to the custom of his day, he visited Abingdon every market day, and prescribed for patients who might wish to consult him. In his house in Oxford he set apart an oratory, in

which he caused to be conducted, daily, the service of the Church of England. Amongst his colleagues he had at this period John Fell, who likewise entered into the service of the King as a soldier, and became an ensign, but afterwards entered the Church, became Dean of Christchurch, later on Vice-Chancellor of the University, and finally Bishop of Oxford. Fell, in combination with John Dolben who became in time Archbishop of York, and Mr. Richard Allistree, who was in later days Provost of Eton College, joined Willis in carrying out the liturgy and sacraments of the Church in the special room of his house to which reference has been made. The services were open to any who chose to attend them.

Willis continued in practice at Oxford during the period which terminated with the death of the King, and during the whole of the Commonwealth. It is a part of the glory of the Commonwealth that it allowed the partisans of Charles to practise their vocations without serious molestation. It is true it turned out distinguished men from official positions, Harvey himself being no exception to this rule; but it did not interfere with private labours of a professional kind, like those of medicine. So Willis went on increasing his practice in Oxford until the Restoration in 1660. On the Restoration, Dr. Joshua Crofts, Sedleian Professor of Natural Philosophy in the university, was ejected, and on August 25th, 1660, Willis was appointed in his stead. The next year he graduated Doctor of Physic, and added greatly to his professional reputation, being summoned, says one of his biographers, Thomas Birch, "to most of the people of quality about Oxford. In one of his journeys in April, 1664, he discovered a famous medicinal spring at Astrop, near Brackley."

Amongst his other friends of importance Willis num-

bered Sheldon, Archbishop of Canterbury, upon whose invitation he moved from Oxford to London in the year 1666. He chose for his place of residence St. Martin's Lane, and very quickly rose into eminence as a practitioner. He was at this time married, his wife being Mary Fell, daughter of Dr. Samuel Fell, Dean of Christchurch, and sister of the famous Dr. John Fell. By her he had four children—one son, Thomas; and, three daughters, Jane, who in the year 1701 married Robert Symonds, High Sheriff for Herefordshire, Rachel, who married Robert Lingen, of Radhook, in Gloucestershire, and Katherine, who died in 1667.

In his mode of life Willis was extremely methodical. He rose in time to go to morning prayer at six o'clock in summer, seven in winter. In London he did not open a room in his own house for services, as at Oxford, but he arranged with the schoolmaster who taught in the Vestry Hall adjoining the church in St. Martin's Lane to read prayers in the church of St. Martin's morning and evening, so that the scholars as well as the general public might attend; and, the plan seeming to answer well, he paid the schoolmaster £20 a year in augmentation of his stipend for the services rendered, leaving him the same for lifetime. To the poor he was a most liberal benefactor, allotting always from his practice a part of his profits for charitable purposes. In the latter part of his life he devoted all the fees which he took on Sunday, and which amounted to more than those of any other day of the week, to deeds of charity. In 1664 he had been made an Honorary Fellow of the Royal College of Physicians of London, and on November 18th, 1663, he was elected a Fellow of the Royal Society, a society then in the days of its birth. He was not, however, admitted to the Royal Society until October 24th, 1667, an omission

accounted for in the Journal Book in the following terms: "Dr. Willis was chosen, having been forgot to be chosen again at the time when, upon the renewall of the charter, the Council, according to the power granted them therein, did receive and admitt into the Society such persons as had been elected Fellows afore upon the first charter."

Willis probably stood at the height of his fame from the years 1667 to 1673, not a very long period in a London life, but brilliant for the time, his table being the resort of most of his eminent contemporaries. He is said to have refused the honour of knighthood, but on what grounds there is no evidence. There is, however, a gossiping statement that the "Merry Monarch," Charles II., chose to make Willis the subject of his mirth, saying of him, in a thoughtless moment, "that he killed more of his subjects than an invading army"; it has been assumed that this observation rankled in the breast of Willis and even shortened his days. Perhaps for this reason he refused the courtly favour that was offered him.

Dr. Munk, quoting from Wood, says that Willis "became so noted and so infinitely resorted to for his practice that never any physician before went before him, or got more money yearly than he." Munk also refers to the peculiar and unfortunate relationships that Willis held with the King's brother, the Duke of York, afterwards James II. of England. James was married twice, first to Anne Hyde, daughter of the Chancellor, the famous Earl of Clarendon, by whom he had eight children, all of whom died except Mary, who married the Prince of Orange and ascended the throne with him, and Anne, who came to the throne as Queen Anne. Two years after the death of his first wife, Anne Hyde, James married Mary, the daughter of the Duke of Modena, from whom came the Chevalier de

St. George, the first Pretender. Regarding the family of James by the first wife, Willis was consulted, with the result recorded by Bishop Burnet as follows: "The children were born with ulcers, or they broke out upon them soon after, and all his" (the King's) "sons died young and unhealthy. This has, as far as anything that could not be brought in the way of proof, prevailed to create a suspicion that so healthy a child as the pretended Prince of Wales could neither be his, nor be born of any wife with whom he lived long. The violent pain which his eldest daughter had in her eyes and the gout which early seized our present Queen are thought the dregs of a tainted original. Willis, the great physician, being called to consult for one of his sons, gave his opinion in the words 'Mala stamina vitæ,' which gave such offence that he was never called for afterwards."

That the opinion of Dr. Willis was sound events proved too faithfully. Poor Queen Anne, daughter of Anne Hyde and James II., gave birth, we know, to child after child (some say nineteen) that were incapable of life. This meant *mala stamina vitæ* without any kind of doubt, but perhaps the stern and honest physician was too outspoken. He cared little, we may be sure, for courtly favour, and, as a staunch Protestant, cared nothing for the favours of an enemy to his faith.

THE WORKS OF THOMAS WILLIS.

Considering the comparatively short time at the disposal of Willis, it is astonishing what an amount of work he performed. He left treatises on eleven subjects: (1) Fermentation; (2) Fevers; (3) Urines; (4) Ascension of the Blood; (5) Muscular Motion; (6) The Anatomy of the Brain; (7) Description and Use of the Nerves; (8) Convulsive Diseases; (9) Rational Pharmaceutics;

(10) The Scurvy; (11) The Soul of Brutes;—this last in two discourses.

As we enter into the study of the labours of Willis it is necessary, for a moment, to recall the condition of medical science and literature in the period during which he flourished. The period was critical. Anatomy had made splendid advances, and physiology had so far progressed, that under Harvey's demonstrating hand the circulation of the blood was acknowledged as a fact beyond dispute. The discovery by Aselli of the "lacteal system" had also been accomplished, and by this time the practice of medicine had commenced to rest on the bases of anatomy and physiology.

Another element of science was now being added. Chemistry, which had for some centuries been cultivated in a sublime obscurity, was at last beginning to take form and shape and to promise to be a guide to the dispensator of curative substances. Willis was one of the early men to recognise this development, and, bearing in mind the few opportunities he possessed and the many mysteries he had to dispel before he could arrive at anything like the light of truth, we may almost consider him the chemist of his time.

Connected with the minuter studies of anatomy there was just at the moment coming into first use the microscope, with which instrument of research he was familiar.

Antony Van Leeuwenhoek, born in 1632, would be about thirty-five years of age when Willis was writing the essays, in which he tells us that he used the microscope. What kind of instrument he used is a mere matter of inference; it was, I think, nothing more than a lens, since he himself defines the word "microscope," in a table of hard words derived from the Greek and Latin,

in the following manner: "Microscope—*A perspective glass to behold minute and very small bodies; a magnifying glass.*" Whatever he used, it answered his purpose well, as his plates bear witness. I should suppose his instrument magnified about forty diameters, and he certainly was one of the first English physicians to use the microscope for medical research.

There is one other point, reserved as the most important of all, connected with his philosophical labours and his discoveries: he was the founder of the physiological anatomy of the brain. He had learned, no doubt, from preceding authors the general anatomy of the brain, but he established the mode of examination of structure in brain, cord, and nerve, which has remained a foundation unto the present hour. We get from his labours terms which carry his name without challenge; witness one only: "the circle of Willis."

How difficult he felt it to be to enter upon the labour to which he subjected himself is most ingenuously conveyed in the close of the Preface of his first medical philosophical discourse on Fermentation, which he gave to the world in 1659:—

"As to our method and manner of philosophising, no man can blame me if I should not here describe all things according to rule and analytic patterns, because in this work it chances for me to wander without a guide or companion in solitary places, and as it were in a solitude trodden by no footsteps, where I not only make a journey, but my way also; therefore, whenever I deviate, I cannot be said to err among right judges of our endeavours, who have no path in which I should walk, nor could find a track which I might fear to miss."

The above observations refer specially to the philosophical bearing of the genius of Willis. It must not be

forgotten, at the same time, that he was a practitioner, and that his works consequently are likely to be enriched with good clinical instructions. His works *are* so enriched, and there are many portions of them of a purely practical kind, for which I must reserve a special section. It is now time to proceed, in a sectional manner, to descriptions of certain of his labours according to the nature of the studies to which he lent his faithful and industrious mind.

ANATOMICAL WORK.

Thomas Willis may be considered, in the first place, as an anatomist; and, as an anatomist, he must be considered as the leading great expositor of the structure of the brain, spinal cord, and nervous fibres. Strange as it may seem to some modern ears, it is nevertheless true that no man, even in these days, can get a sound and deep knowledge of the cerebral and nervous structures unless he first master this master, and learn how this master set himself to work in his labour as told by his own pen. He determined "to rely on this one thing: not to pin his faith on the received opinion of others, nor on the suspicions and guesses of his own mind; but, for the future, to believe nature and ocular demonstrations." Therefore he "betook himself wholly to the study of anatomy," and did "chiefly inquire into the office and uses of the brain and of its nervous appendix." He addicted himself "to the opening of heads of every kind, to inspect as much as he was able, and from the contents to study the exercises, defects, and irregularities of the animal government, that so a firm and stable basis might be laid on which not only a more certain physiology than could be gained in the schools might be laid, but on which also the pathology of the brain and nervous stock might be built."

In his anatomical work Willis was, fortunately, aided by two of the most remarkable men of his time, namely, Dr. Richard Lower (*tuberculum Loweri*) and Dr. (later on Sir Christopher) Wren, then Savillian Professor of Astronomy at Oxford and afterwards the immortal architect of St. Paul's Cathedral. To the aid he received from these men of splendid genius he was, he tells us, much indebted. Lower was indefatigable in the dissection of the nerves, and Wren, with his own hands, executed many of the delineations with which the dissections were illustrated. In addition, Dr. Thomas Millingen and Dr. E. King lent their thoughtful and able assistance, the whole seeming to form what we, in these days, would call a committee of inquiry into the greatest of all the problems of the animal economy and some of the highest problems in the whole domain of natural science.

Anatomy of the Brain.

In describing the brain Willis naturally commences with the enveloping membranes. Following Hippocrates and Galen, he recognises two membranes only, namely, the dura mater and the pia mater. At the same time he seems to think that the dura mater has two surfaces. Before his day there had been a contest on this subject. Realdus Columbus had declared for two upper membranes, asserting that he had separated the dura mater into two parts. To this statement Laurentius had replied that, though there seemed to be two parts, there was really only one, adducing in support of his argument that, although there were two layers of peritoneum, there was only one membrane. Willis, without entering into the discussion, appears, in a certain sense, to have entertained a similar view, for he assigns to the dura mater two distinct duties: one of protection by its outer, the other of

secretion by its inner surface. He describes the anatomy of the membranes, dura mater and pia mater, with great skill and care, and his account of the choroid plexus as a kind of lamp suspended in the chambers of the brain is at once as practical as it is poetical.

The mode in which the brain is fed with blood, and the picture of the arterial circuit to which his own name has been always attached since the publication of his essay, is felicitous, as is the description of the return of the venous blood through the sinuses and the mode in which, delayed in its course, it is so perfectly regulated that there shall be time for secretion of the water of the brain, and that without any undue pressure. The vessels are defined by the pencil of the artist, as well as by the pen of the writer, and little has remained for the future anatomist to do in the matter of amendment of this exposition of the cerebral circulation. The division of the cerebral nerves formulated by Willis is practically the same that holds good at the present hour. He gave as divisions nine pairs of cerebral nerves, although he is generally credited with giving ten. His divisions into pairs are—(1) the olfactory; (2) the optic; (3) the motores oculorum; (4) the pathetic of the nerves of the eye; (5) the fifth pair, "having a large province," distributed to the mouth, palate, and face; (6) the sixth "a pair of small nerves going to the ball of the eye"; (7) the seventh pair "the auditory or hearing nerves, and their two processes on either side of them"; (8) the eighth pair the wandering pair, consisting of many fibres; (9) the ninth pair consisting also of many fibres, which, tending downwards, grow together into one trunk. To these he adds one more pair, the tenth, "consisting of many fibres and carried to the muscles of the neck." "But," he says, "it is doubtful if this pair ought to be

called the last of the skull or the first of the vertebrals." The division, throughout, is excellent, and although we have modified it by linking together the glosso-pharyngeal, the par vagum, and the spinal accessory as the eighth pair, and by making the hypoglossal the ninth pair, it is exceedingly doubtful whether we have not diverged for the worse in departing from the Willisian classification. When we come to the description he supplies of the brain substance itself, it is curious to find the cerebrum defined as the brain, and the smaller brain, carefully depicted with the tree-like figure of the arbor vitæ, as the cerebel. The medulla oblongata is defined as if it had been specially detected as a distinct part by the anatomist himself. The medullary and the cortical structures of the brain are differentiated, and many of the individual parts that make up the mass of the cerebrum are named. The divisions of the brain, we are told, are—"(*a*) the two hemispheres and the two lobes or partitions of either; (*b*) the narrow cranklings, turnings, and windings, or the gyrations and convolutions, or rolling together of the brain; (*c*) its double substance, viz., cortical and medullary; (*d*) the common basis of all, viz., the callous body; (*e*) its subtension or fornix; (*f*) the appension or circuit of the brain over and above the oblong marrow; (*g*) and, what results from thence, the void space or ventricles made by its infolding together."

The description of the hemispheres of the larger brain, called by Willis the brain, is very ably supplied, if quaint and somewhat involved and figurative language be excused as peculiar to the style of exposition current in his school. He gives excellent directions for dissecting the brain, and expatiates on the different parts with a keen view to function. Then from the brain proper he passes

to the oblong medulla, which he looks upon as a wedge-like centre of intercommunication between the posterior base of the great brain and the cerebel, or little brain, and the spinal cord or marrow. He shows himself to be conversant with the pons, or protuberance which binds all these parts together, and he is particular in describing the medulla as the base with which the hemispheres of the big and little brain are connected. The position, form, and structure of the cerebel are pointed out, and the relationships of the cranial nerves to the cerebel are thoughtfully defined.

When he comes to the anatomy of the spinal marrow and to the distribution of the nerves throughout the body, Willis is seen at his best. He had here to help him the skilful hand of Richard Lower, and the advance in anatomical science which was brought out by their labours must have come as a revelation. The first four pairs of nerves arising within the skull are made the subject of a separate chapter; then the origins and courses of the fifth, sixth, and seventh pairs are unfolded; and from them attention is turned to the eighth pair, the par vagum, the wandering nerve. It is but fair to preceding anatomists, Vesalius especially, to say for them that they had paved the way towards a correct investigation of the course of the par vagum, and that they had given it the name of "the wanderer." But the admission does not detract in the least from the part which Willis took in the field of further discovery; on the contrary, what he has written on the subject shows how much he was able to clear up and take out of doubt. His predecessors had mixed up the course of this nerve with the branches of other nerves that joined with it in its ramifications. Willis corrected the error and traced the nerve throughout all its course by its own line of fibres. He follows these fibres down-

wards in their course to the heart and to the pulmonary organs, says the fibres are distributed together with the blood-carrying vessels through the whole substance of the lungs, where in their passage, step by step, they run with the pipes of the bronchia, the arteries, and the veins; and, many shoots being set forth on every side, they climb upon and compass about these vessels. The trunk of the wandering nerve, descending on both sides, nigh the sides of the trachea, distributes also many shoots into the coats of the œsophagus, and in the further descent of the fibres of the nerve into the stomachic region distributes itself to the viscera.

The course of the spinal accessory nerve is followed with equal care; and, with much nicety, the nerve of the diaphragm is traced from its cervical and brachial origin down to the diaphragm. The work on the nerves concludes with a chapter, finishing the series rather abruptly, on the blood-vessels to the spinal marrow.

OF THE ORGANS OF BREATHING AND THEIR USE.

The anatomy of the respiratory organs is given by Willis as a part of his treatise on the operation of medicines on human bodies, or *Pharmaceutice Rationalis*, and is another effort remarkable for its accuracy, and for the beauty of the plates with which it is enriched. The chapter remains to this moment a valuable contribution not merely to the general anatomy, but to the minute anatomy of the pulmonary organs. The descriptions of the lung tissue, of the pleural covering, of the lobes, of the lobules, of the two circulations, and of the nervous supply, are admirable; but it is when we come to the minute structure that our admiring surprise is most pronounced. The preceding work of the illustrious Malpighius is freely employed to illustrate the essay; but other work, strikingly original, is

also added. The vesicular structure of the lung tissue is finely depicted, and a plate of the lymphatic ducts "creeping through the superficies of the lungs, as seen in the warm large lobe of the ox and expressed to the life," could not be surpassed by our modern artistic skill. The four coats of the trachea and of the bronchial tubes are explained; the circular muscular fibres forming the contractile muscular coat are traced down to the finest ramifications; and the stronger muscular organs which work to accomplish the acts of breathing are equally well expounded. In brief, the more carefully we read these pages on anatomy, the more we are impressed with the advanced learning and fine discrimination of the author.

THE PRIMÆ VIÆ, OR FIRST PASSAGES.

The description of the uses and affections of the part in which medicines first begin to operate—namely, the alimentary canal—is not so striking as that on the respiratory system. Still there is in this instance also much that is original and progressive. All the intestinal organs receive their proper share of attention, and the stomach is depicted in plain and simple style. The muscular coats of the stomach, longitudinal, circular, and oblique, are well drawn. The outer coat or covering, with its vascular network, is exhibited; and the inner or mucous coat is also exhibited so as to display the folds, or, as we call them, rugæ, the nervous and vascular supplies, and the glandular surface.

The anatomical labours of Willis, thus briefly referred to, might be largely extended; but enough has been rendered to indicate the industry and care with which he proceeded in his task. That there are many and serious omissions must be admitted, as, for example, the omission of the liver, the kidneys, and the pancreas, about the

kindled, into every part of the whole body, and might then receive the burnt and half-extinguished blood, returning from the whole body, imbued with new and inflammable juices, and might deliver it to the lungs to be rekindled.

How well the heat-giving course of the blood is here described will be manifest to every reader who has studied the subject; but even that description is surpassed by another in which the function of the heart is defined and limited. The heart, although a mere muscle and exercised only with an animal motion, seems, he teaches, to serve alone for the circulation of the blood. But it does more: it so much helps to moderate the ascension of the blood and its burning under the rage of the passions, and so directs the other works and uses of the animated body, that we have thought the vital or fiery part of the soul must have its chief and, as it were, imperial seat in the heart and lungs in every distemper or affection, as of grief, joy, or fear, and also in paroxysms of disease. Hence it happens that the blood flowing into the heart fluctuates and is enkindled with divers impulses. The argument is subtle, and even to this day a novel reading of cardiac disturbances from external impressions and impulses as well as from internal perturbations and storms acting on the blood in its circuit. The heart is a mere muscle: it does not itself feel; it is a machine which regulates movement, and, by its passive movement when left to its own tasks, it is independently mechanical. But bring unnatural impulses to bear upon it, force it out of its daily course, and then it becomes a disturbed centre that "feels" to be the cause of the disturbance. It is not the cause, but it feels to be so, and, as a consequence, we say, unwittingly, it is the cause. So an ignorant person looking at the perturbations in the balance-wheel of a timepiece would attribute, knowing nothing of the mainspring, any aberration

in the going of the piece to the balance-wheel, as the primary cause of the aberration.

The Animal Fire and Muscular Motion.

He treats through many passages on the subject of the animal fire. He admits that blood will not burn like a lamp, but he considers this no objection; for does not rotten wood burn without manifestation of flame, by a slow burning, and why not blood? In burning, blood too may give off a smoke or air of combustion, although it be an invisible air. Life, in short, is a combustion, a living fire, and the blood is a living fluid, in the living body, so long as it is in motion.

The chapter on muscular motion is full of interest. In order to have muscular activity, there must be three factors:—firstly, the original of the action, or the first designation of the motion to be performed, that which proceeds from the brain; secondly, the instinct or transmission of the thing begun to the motive parts, and which is through the nerves; thirdly, the motive force itself or exertion of the spirit implanted in the moving parts either into a contractile or an elastic force. As to the original or beginning of the muscular motion, which proceeds from the brain, that is one of two kinds, which may be called voluntary or involuntary. If the origin be in the brain proper, namely, from the great hemispheres, the motion is under the will or voluntary; if the origin be from the cerebel or little brain, it is involuntary, the function of the cerebel being to act as the centre where the law of nature resides, and to govern such natural acts as respiration, arterial circulation, intestinal movement, and all such functions as are "according to the solemn rite of nature."

Respecting the phenomenon of contraction of muscle

he makes some admirable observations. He says that as often as the motion of a living muscle was beheld by him he could conceive and collect no other thing than that certain elastic particles did rush into the fleshy fibres from the tendinous ends and did intumify and force themselves nearer to each other, or together; then, the same particles presently coming back from the fleshy parts towards the tendons, the relaxation of the muscle happens. In a bare muscle when he had separated every fleshy fibre or a company of them, apart from the rest, he could plainly discern by the microscope the tremor begin at either end of the flesh, and pass towards the middle. Further, each fibre being tied about the middle, while yet compacted with the others, was contracted or drawn together; but a ligature being placed round it, near towards its two ends it remained flaccid between the two ligatures. He then placed two ligatures at equal distance between the middle and the ends of the same bundle of fleshy fibres, which being done, a rising and contraction from the tendinous ends took place up to the points ligatured, but went no further, the middle part between the ligatures remaining unmoved and flaccid.

Physiology of the Nervous System.

It is to be expected that one who paid so much attention to the bare anatomy of the nervous system, as did our present anatomist, should have much to say about the physiology of the brain and its subordinate parts. He has much to say, enough to form an essay of itself, instead of the brief abstract I am able to present. Of the brain, meaning what we now designate the cerebrum, he declares that he accounted it the chief seat of the rational soul in man, and of the sensitive in brute beasts, and indeed the chief mover in the animal machine, the

origin and fountain of all motions and conceptions. But some functions do chiefly and more immediately belong to the substance of the brain, while others depend, as it were, mediately and less necessarily upon it. Imagination, memory, and appetite, he conceives, depend immediately on the brain proper. The rest of the merely natural faculties, as sense and motion, also the passions, and instincts, though they depend in some measure on the brain, yet they are properly performed in the oblong marrow (medulla oblongata) and cerebel.

He had a very good notion on the functions of the convolutions. He looked upon them as folds for the extension of surface of the cortical substance. The "anfractious or crankling brain" is, he thought, "like a plot of ground planted with nooks, and corners, and dauks, and molehills, for a more ample extension than if its superficies were plain." He explains how to unfold the convolutions by removing the pia mater, and he suggests that the involutions of the pia mater, by dipping deep down into the cortical matter, supply the cortical matter the more freely and copiously with blood. Of the two kinds of brain substance he speaks correctly. The cortical is the receptive surface, the surface which receives impressions; and the medullary, or white substance, is the conducting structure from which the conveying nerves proceed in their course. He was conversant with the fact that the arterial ramifications are attended throughout by nervous filaments; and although he does not appear to have dissected out the sympathetic ganglionic system, he treats of ganglia as if he had some inkling of a ganglionic chain connected with the cerebro-spinal nervous network in the viscera.

The Nerve of the Diaphragm.

There are numerous other passages of a physiological nature which deserve to be introduced into this section on the works of Thomas Willis. I must be content with one other extract as illustrative of the ingenious reading of this, in his generation, unique scholar. He is treating on the functions and uses of nerves, and is brought to the explanation of the nerve which he calls "the nerve of the diaphragm," the phrenic, as we now call it; and thus he reasons. We ought to inquire concerning the nerve of the diaphragm why it always proceeds from the brachial nerves and why it does not rather arise immediately from the spinal marrow. The reason, he thinks, is that the motion of the arms must have some reciprocal connection with the motion of the diaphragm by a certain respect or habitude. The arms or fore-legs of all creatures are made for labour or hard exercise. But under too much labour the act of respiration is very much increased, so that the breath almost fails and is in danger oftentimes of being lost, because too much blood is forced into the heart, and would oppress it, if frequent and laborious respiration were not instituted. Therefore, that the exercise of the body may be regulated according to the state of the præcordia, that the motion of the arms may observe the action of the diaphragm, it is provided that the nerve of the diaphragm should be tied, as if it were a bridle, to the brachial nerves, and so timely warn them, if unmindful of their duty, and as soon as the breath begins to fail command them to desist from further moving the body. So when cattle are overdriven, urged beyond their strength, there follows, in them, injury of the heart or diaphragm; and when horses are driven too rapidly with the stomach loaded, there follows permanent difficulty of breathing,

because the nervous tone of the great respiratory muscle is broken.

PRACTICAL AND THERAPEUTICAL WORKS.

We have seen that Willis was considered by the world in which he lived in the light of a renowned practitioner of the healing art. It is quite certain that he was fond of the practice of his profession, and it was for the advancement and practical development of it that he resorted to anatomical and physiological pursuits. As the work of William Harvey on pathology was unfortunately lost, we cannot tell how far Harvey may be credited with original quality of practical skill; but we have the best evidence that Thomas Willis was in the most eminent degree qualified in the treatment of disease, and that, as far as his lights were bright, he stood in advance as a bearer of them. It was the one grand object of his labour to place the art of medicine on a rational basis. He learned structure, he learned function, that he might be sound in the detection of disease, in the symptoms which mark out the courses of disease, and in the methods by which diseases may be prevented or cured. The physiological process is the introduction to the practice. That is the rule wherever he speaks either on prevention, nature, or treatment of disease.

In every sense he was a general physician, but, unlike Harvey, he seems to have held himself to the practice of medicine as distinct from that of surgery. The surgeon would be to him a skilled hand whom he might call in to his aid, as he would have called in a mechanic to repair his timepiece or his house. We deal, therefore, with him as with a physician pure and simple, the head of his profession in the time of the Commonwealth and

immediately after the Restoration; a physician accomplished in the theories of his day and skilful in practice.

On Fevers and Agues.

He has a work on fevers, and specially on the fevers called intermittents or agues. They were the rich fields of practice, both for the great physician and for the poor apothecary. His theory about all fevers was that the pyrexial condition is a condition of ferment or fermentation. The zymotic theory of febrile diseases is not more firmly engrafted into the mind of the physician of 1892 than it was into the mind of Willis in 1662. Every paroxysm of ague is a fermentative perturbation, and the symptoms are so well described, a student still at his studies might learn the history of the affections in all their typical forms as well from Willis as from the most modern writer. As to treatment, what he tells us is memorable because it introduces to us the then newly discovered remedy for agues, Peruvian barks. He had been accustomed to treat ague most usefully by emetics, with a favourable idea of an empirical plan of firmly compressing the radial arteries as the paroxysm was approaching, so as to lock out the circulation from a part of the body, and retain the blood there for a time. But later the use of the Peruvian bark came before him, and he began to try its value. It did not, in his opinion, prevent the recurrence of the paroxysms at once, but it was the only "alexiterion or corrective poison" for a quartan fever. He administered, for a dose, two drachms of the bark infused, for two hours, in sack or white wine in an open vessel.

The descriptions left by Willis of the pestilential and putrid fevers which were so prevalent in London in his day, and his accounts of the camp fever which affected the armies both of the Parliament and the King, are so

absorbing, historically, I dare not here enter on them lest I be led into too long a story; but I must refer briefly to his "Description of a Catarrhal Fever epidemical in the Middle of the Spring in the Year 1658." The spring coming on, an intermittent tertian fell upon some. About the end of April a distemper arose suddenly as if sent by some blast of the stars, which laid hold of very many at once, so that in some towns in the space of a week above a thousand people fell sick together. The first symptoms were those of a catarrh, accompanied with a feverish distemper, joined with heat and thirst, want of appetite, a spontaneous weariness, and a grievous pain in the back and limbs. The fever, however, was more remiss in some, so that they could go abroad and follow their affairs in the time of their sickness, but complaining, in the meantime, of want of strength and of languishing, a loathing of food, a cough, and a catarrh. Some were afflicted with much more severe symptoms, and many died; but those that died for the most part died by reason of the strength being leisurely wasted, and a serous heap gathered more and more in the breast with the fever much increased, and with a difficulty of breath, as in those sick of a hectic fever. The disease was specially severe and fatal to persons who were advanced in life, and who were enfeebled, while the more strong and those of a healthful constitution recovered. Concerning the disease, he wonders what "procatartic" cause it had that it should arise in the middle of the spring suddenly, and that the third part of mankind should be distempered of the same in the space of a month.

The disease thus described was, of course, none other than the disease lately raging amongst us, which we call influenza. We could not even now draw a more faithful picture of it, nor could we perhaps give a more lucid

hypothesis of its cause, although it be admitted that the hypothesis is too vague to be quite satisfactory. As to treatment, he teaches that when the disease is slight no treatment of an active kind is necessary, because it is the tendency of the affection to get well of itself; but when the affection becomes serious, when the breathing is oppressive, and when the heart is burdened with blood and enfeebled, then active measures must be taken; and the circulation must be relieved by the abstraction of blood, and by the administration of diaphoretic remedies, "for the vessels being emptied by this or that means, both the immoderate heat of the blood and the abundance of the serum are restrained." Not a bad argument for a physician physiologist, who saw that liberating a strained organ, even by removing blood from it, might be the surest mode of giving quick relief. We should relieve an inanimate engine in that manner now, and successfully.

Intermittent Pulse and Circulation.

Diseases affecting the heart and lungs occupied closely the time of Dr. Willis, and all that he writes on these diseases has a value of its own not easily described. Why he did not discover mediate auscultation is a marvel, for he was near to it. He listened, at a distance, to the beats of the heart, and commented on what he heard. He was conversant with the phenomenon of intermitting pulse, and distinguished intermission from palpitation. He was also of opinion that there may be exceptional instances in which an artery may intermit in its beat whilst the heart itself is not distinctly intermitting; but he held, as a general rule, that the intermission of the pulse is due to the intermission of the arterial propelling stroke of the heart. The pulse intermits because the contraction of the heart is, for a time, suspended, and the

"pause or rest" of the heart is twice as long as it ought to be. This defect of the heart is, he says, in its nature various. Sometimes the intermission occurs regularly, say at the third, fourth, or fifth beat; sometimes it is uncertain or fleeting. The vibrations of the artery are usually strong and brisk enough, but occasionally the first beat after the intermission is strongest, the next one less powerful, and so they grow less and less, insensibly, until the intermission occurs again. The cause of the intermittent pulse depends, he thought, not upon the blood, nor upon the heart itself, but upon the irregular conveyance of the animal spirits out of the brain into the nerves belonging to the heart; "wherefore when the stock of them is deficient the motion of the heart ceases ever and anon, for one beat, till, the supplies of spirit being reinforced, its action may be renewed." In illustration he says, "I have seen a mill that was driven about by a small stream when the water fails, and is almost exhausted, stop for a little while, and then, when the stream rises, immediately repeat its rounds and go on again." Was ever a more perfect or more simply expressed explanation, of a heart failing from nervous default, given to the world? When the intermittency is very marked, they who suffer from it may, he continues, experience headache and giddiness, and may often be subject to what is called nightmare, and sometimes apoplexy. Yet as a common thing the affection is inconvenient rather than dangerous. In its treatment diet plays an important part, and, in the autumn, especially, a small course of physic should be prescribed. Such is his wise instruction.

Coughs and Diseases of the Lungs.

Concerning diseases of the lungs he sets out by saying that cough has for its first cause an irritation of

the nerves belonging to the lungs, which irritation may be from the bronchial surfaces, from the lungs themselves, from nerves at a distance but in communication with the nerves of the lungs, or, from a central disease or irritation in the brain itself. He is very full on the subject of consumption of the lungs, emphasising the doctrine that it descends by heredity, and yet showing that it may be kept from development, even in the predisposed, by avoiding impure and overcrowded air. Some of his means of cure are crude enough, as, for example, when he prescribes snails and snail syrup; but there is one line of treatment, by way of inhalation, which he names, and which ought not, in these days, to be forgotten. He orders the inhalation of the vapour of burning sulphur, combined with frankincense and mastich, to make a *troch*, that can be burned and the fumes inhaled. Again, in the same direction he prescribes arsenical inhalation, by means of a *troch* consisting of:— White Amber and Olibdanum, of each two drachms; Prepared Orpiment (sesquisulphuret of arsenic), half an ounce; Styrax and Labdanum, of each one drachm and a half; Solution of Gum Tragacanth, sufficient to make a *troch* for fumigation. One more curious word on this head. He tells us that it was a custom, sometimes attended with good success, "to prescribe the smoke of arsenic sucked into the mouth, like tobacco kindled in a pipe; or to burn, like tobacco in a pipe, little bits of cloth stained with arsenic (such as wherewith the walls of taverns are hung), and to suck the smoke into the consumptive lungs for cure."

It would afford me much pleasure, and give, I think, equal pleasure to readers, to follow out much further the practical part of the work of our present author. He has an essay on Scurvy, which brings into a kind

of new life one of the too common diseases to which our forefathers were subjected. He has also an essay on "Gout," another on "Convulsive Diseases," another on the "Operations of Medicines on Men's Bodies," and more still that would yield excellent mental food; but were I to go on, I should not know where to stop, and so I must even rest here from what is so interestingly practical, and come to a final page bearing on what some consider the noblest part of this Master, namely his part as a natural philosopher, as well as a physiological physician.

PHILOSOPHICAL WORKS.

"DE ANIMA BRUTORUM."

The great and what we may truly call the philosophical work of Willis consists of his treatise concerning the *Souls of Brutes*, and is known amongst scholars as the *De Anima Brutorum*. The book was written as a diversion from a great affliction, the death of his "dear wife." In its composition he was much helped, being himself almost always interrupted through practice, by the famous and skilful anatomist and physician Dr. Edward King, and by his intimate friend Dr. John Masters, "skilful in physic and anatomy." The treatise is divided into two parts; the first is physiological, showing the nature, parts, powers, and affections of the soul; the second is pathological, unfolding the diseases which affect the soul and its primary seat, namely the "brain and nervous stock." The whole is dedicated to Gilbert, Archbishop of Canterbury (Dr. Sheldon), and to the Vice-Chancellor, Doctors, and Masters, of Oxford, "who diligently profess, greatly adorn, and happily promote, good letters in the most famous university."

It is obvious from the tone in which this treatise is

introduced and carried out that Willis looked upon it as his chief endeavour, his one grand and distinctive cast in literature, as well as science. It is touched, all through, with infinite care, and the copper plates with which it is enriched are worthy of modern art. It is, moreover, a glance at the whole economy of living things to the extent knowable by him and his compeers, a glance no man need be ashamed of in any age. In it, industry unites with insight, and breadth of knowledge with depth of understanding. To all he has added splendid learning, mastery of what has gone before, fine criticism, and generous acknowledgments.

The argument of the book is that in both man and brutes there is a sensitive or corporeal soul, which is of the same nature and quality in man and brute, but that man is endowed with another and distinct soul, called the mind, or rational soul, a something higher and eternal. The corporeal soul belongs to the flesh, and leads men, like lower animals, to sensual pleasures; whilst the rational soul, being helped by ethical rules or Divine favour, invites to good manners and to works of piety. But the corporeal soul can torment the flesh, and extending its influence, can even torment the rational soul, so that "the flesh lusts against the spirit, and the spirit against the flesh," as Plato and Paul have each declared in their respective ways. Most curious it is to see how near he always sails towards our own fresher and yet older knowledge, and how, while holding by the ideas of his time, and influenced by them, he puts forward what have been the discoveries or re-discoveries of later men. In his chapter on the species and differences of brutes according to their different parts, he shows himself a good comparative anatomist; and, in a description of the earth-worm, he shows how careful he can be in the comparisons he

draws on the various constructions of animal bodies. The plates also—copper plates—are notable for their fidelity. As a cerebral anatomist and psychologist he stands a head and shoulders above all his compeers and many of his successors. Gall himself might almost have envied him in his delineations of the diverging fibres of the brain. It is, however, on the question of the two souls that Willis is most impressive; and although we, who now understand that the higher intelligence of man over the brute flows from the nobler and higher development of the brain of man, and that man is dual because he has two cerebral hemispheres—although we can see where he probably missed his way, we can equally well admire his fine and subtle application of the hypothesis he introduced. It is not a bad working hypothesis, and for one who lived when belief in witchcraft and cures by royal touch were accepted as real truth, what he advanced was consistent on the precedents of the case, and logical. The man, so he argues, has two souls, the corporeal and the rational; the brute has one soul, and none other, namely, the corporeal. But the corporeal soul governs a long and necessary range of corporeal acts. It is the soul of the passions; it is the soul that perceives; it is the soul that excites appetites and mere mechanical, rhythmical, and automatic actions; it is, in a sentence, the animal as distinct from the reasoning soul—soul of ratiocination. It influences the body, and makes the body do what is, or may be, visible to other souls looking on. But it does more, or it may do more. It may interfere with, or conquer, or be subservient to the reasoning soul. Here, therefore, is the dual man, here the distinction between the reasoning mind and the mere automatic mind. He refers to a youth—a fool—living in his neighbourhood, who, although silly and foolish, yet knew exactly the interspaces of the hours,

and, like a clock, called out each hour at the right moment. This was the automatic voice of the sensitive soul in one in whom the reasoning soul did not exist, or existed so faintly that it was without influence, as it is in lower animals, which, by repetition, do the same acts over and over again, until they become automatic in their actions.

The sensitive soul is the most powerful, according to this argument, in the majority of mankind, but sometimes this is not the fact. The rational soul may dominate and keep the animal soul in feebleness. Hence strong and wise men are not begotten of strong and wise men. In this respect, as if for a safeguard, Nature brings man down to the animal for his new life. "When the rational soul becomes greatly solicitous in bringing forth its child, the work of the intellect, then the corporeal soul, being called away to wait on the rational, becomes, not at all, or at most weakly, prolific. Again, a body in which the corporeal soul is brought low from old age, drunkenness, effeminacy, or privation, is a body that begets only languishing and unhealthy children. These are the births of stupidity which cannot be helped; so to be born of parents who have a sound mind in a sound body is far better than a large patrimony."

THE LAST ENEMY.

We have seen that Willis came into metropolitan life and practice in the year 1666-7, and that he attained great repute as a practitioner. It was not to be for long. Domestic sorrow befell him, and his was not the mind to bear trial with continued strength of body. His daughter Katherine died on September 30th, 1667, and his son Thomas falling ill with signs of pulmonary consumption, had to go to Montpellier, then the Mentone of England, for the restoration of his health.

Then, alas! the good and loving wife next showed indications of pulmonary disease. For her sake he would have left London, but she would not permit so great a break in his work. She died on the Vigil of All Saints, 1670, and was buried in the North Cross, near the door entering the Choir, in Westminster Abbey. From that date his own health began to decline, but he remained at his labours until the middle of November, 1675, in which month, on the twenty-fifth day, he succumbed to pleurisy and peripneumonia, the effects of an epidemical catarrh then prevailing, probably an influenza. From the Rev. Dr. John Fell, who was the brother of Mrs. Willis, we have a touching little narrative of Willis in his later days. Just before his death he had finished the second part of his *Pharmaceutice Rationalis*, and had written the preface. While this was printing, the sad message arrived that "the author most worthy of immortality, oppressed by the irresistible assaults of a pleurisy, departed from among the living." To the preface of the work, therefore, Dr. Fell added a fraternal postscript, in which is given a brief sketch of the early life of the anatomist and the final scene of his distinguished career. In this postscript is recorded the fact of a legacy which Willis left to St. Martin's Church,* and of the other generous gifts he made to the poor.

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* Touching this legacy I am deeply indebted to the present learned and most esteemed vicar of St. Martin's, the Rev. J. F. Kitto, M.A., for the following reply to an inquiry I made of him on the subject:—

"ST. MARTIN'S VICARAGE, CHARING CROSS, W.C.,

"*February* 14*th*, 1892.

"MY DEAR SIR,—

"Dr. Willis's contribution, or rather legacy, was a rent charge of £20 upon some property in Buckinghamshire, and is still paid. Formerly it was paid to the Master of Tenison's School for performing

"From the beginning of his youth to the last period of his life," says Dr. Fell, "he" (Willis) "was master of no sum of money he accounted his own until he had consecrated some considerable portion of it to God and the poor; and when, a few days before his last, we conferred together, as if he had foreknown his approaching death and being more solicitous about the poor than his own offspring, he diligently advised about stating these accounts."

Undisturbed in adversity, temperate in prosperity, modest in his highest fame, prone to forgive injuries, candid and ingenuous in the profession of his art, indefatigable in his studies, and sparing in his speech, he commended in his last hour, Dr. Fell tells us, "his pious soul to God, having his senses entire to the last breath, and finishing his most exemplary life with the like death." He too was buried in Westminster Abbey, near his wife, "morum suavitate insignis, summo omnium, ac imprimis marito."

A FINAL RECAST.

I bring to a close this imperfect notice of Thomas Willis with true regret, as if I were leaving a friend, beloved as well as honoured. What a faultless life! What a brilliant life! Yet since his death he has had his detractors. A Dutchman, one Schelhammer, in 1684, ventured to attack the work *De Anima Brutorum*, but the shaft fell harmless. Hutchinson in the *Biographica Medica* (1799) presumes also to be critical, arguing that

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service daily in St. Martin's; now it is paid to the vicar for providing daily service.

"I believe that the daily service has been held regularly ever since.

"Yours faithfully,

"J. F. Kitto.

"I think this was not his only benefaction to the parish, but I am not sure, and if there were others they are lost now."

although the works of Willis show the greatest ingenuity and learning, very little knowledge is to be drawn from them, and that perhaps no writings which are so admirably executed and prove such uncommon talents in the author were ever so soon laid aside as the works of Dr. Willis. So much for a commentator who did not understand the works, and who had not the penetration to recognise that they were laid aside simply because they were so much in advance of their day! There were few who had the industry to master them, or the intelligence to grasp the splendid and in many parts unmatched interpretation of nature which they unfold. In reality Willis ranks amongst the greatest decemvirate of physic, and is not second to any one of that immortal band. He was the true founder of a school of scientific medicine which, with many fluctuations, has lasted until the present time, a school whose prime aim it is to find out the causes of disease, and which considers it to be the highest skill, not only to find out causes, but to remove them altogether; a school also which looks on pathological as aberrations of physiological conditions, and which, in the art of prescribing for the sick, would like to learn the correct art from the correct scientific knowledge of the physiological action of every remedy that stands for cure.

ORIGINAL RESEARCH, EXPERIMENTAL AND INDUCTIVE.

"Dire n'est rien; faire est tout."—RENAN.

ON PHOSPHORUS EREMACAUSIS, AND ON AN ATTEMPTED SYNTHESIS OF PUS FROM NERVOUS MATTER.

IN preparing the essay published in last ASCLEPIAD on "The Cerebro-spinal Axis as a Thermal Centre and Water Power," I began, as readers of that essay will find, to experiment with phosphorus in a novel manner, namely, by using it, in a state of refined distribution, as the starting point for the oxidation of organic substances, and specially of cerebral substance, cortical and medullary. The practice thus originated has opened up a new field of inquiry, which bids fair to yield varied and useful results, results sometimes altogether unexpected. Hitherto physiologists, in treating of phosphorus as an elementary constituent of animal bodies, have thought of it in two directions only: as a constructive element and as a toxic element. By its presence in brain structure and bone structure, it is seen to play an important part in composition of tissue; and by its constant presence in many of the colloidal structures, it is assumed to have some important but undefined use in their organisation. Again,

by the effects which it produces on living animal bodies when introduced into them in toxic proportions, it has been found to interfere with the chemical changes natural to organic function, and to induce degenerations specific in character. The late Professor Köhler, of Halle, in an essay on this last-named topic,* followed up these researches of tissue degeneration, tracing out carefully the phosphorus fatty degenerations and those especially which are presented in the liver. Several of us engaged in recording the diseases incident to industrial pursuits have also described the phenomena of phosphorus necrosis; and Dr. J. Mertens in 1860 reported an instance in which phosphorus applied to a wound caused a peculiar gangrene, like the gangrene of aged people, so that he was obliged to amputate the fingers of a hand in which this disease had been produced. The late Dr. Stokes, of Dublin, has also detailed the particulars of a case of phosphorescence of the breast of a woman, in which the phosphorised structure gave out a sufficient luminosity to enable small print to be read by it when all the surrounding space was in darkness.

But these observations, instructive as they are, have no relation to the question I have been led to ask. The question that came to my mind was whether phosphorus mixed in intimate and infinite subdivision with organic substances, or with inorganic substances that will undergo oxidation under favourable circumstances, will oxidise in the mere presence of oxygen and by that oxidation set up further oxidation in other oxidisable elements, and so by eremacausis lead to changes similar to, or identical with, those changes which we call vital.

In the paper on the cerebro-spinal axis I touched, in the briefest way, on this point: in the passage where it is

* An essay he was so good as to dedicate to me, shortly before his early and lamented death.—B. W. R.

suggested (section 8 of the summary, p. 386) that there are two distinct combustions going on in the animal body; one, the central or nervous combustion of low tension and pressure; the other, the combustion which yields the sensible heat of the body, and which is of higher tension, but which is nevertheless probably dependent on the central combustion for its continued existence, for its being kept alight. As if, to explain now more simply, the ends or terminations of nerves were the sparks or points of light by which living combustion is maintained; and as if the animal fire of a part would go out if the nervous cord feeding the part were entirely cut off.

Nature and Method of New Research by Phosphorus Oxidation.

This new inquiry, when once the idea of it is conceived, is simple enough as a research. By mixing phosphorus, in refined and infinitely fine distribution, with numerous inanimate organic bodies—carbons, in some conditions, or hydrocarbons—and then bringing the mixture into the presence of oxygen, it is easy to create a spontaneous combustion by which a motor force is obtainable. What then if this same combustion were excited in complex organic compounds? Such an experimental pursuit has a good basis to rest on, and could hardly fail to lead to some curious and useful result, whatever that might be; and on that basis I have pursued my way.

That phosphorus, in combination with oxygen and with metallic bases, enters the body with food, and is distributed into the tissues for various uses, is a well-known fact; but the modes of its distributions and the reductions it undergoes form a subject of inquiry that has never as yet been carried out. This would be an analytical experimental study of the utmost value, and

one I shall be inclined to follow up. For the present I have to refer to a synthetical research to ascertain what part phosphorus may play as an excitor of chemical decompositions in organic substances by virtue of its affinity for oxygen; a kind of repetition of a course I once took with sodium and potassium, when by setting them free from their ethylates in moist animal tissues I produced destructive changes of tissue and new products, and so arrived at a novel method of treating some cutaneous diseases, like vascular nævus.

The method of research in respect to phosphorus has been of the simplest character. Phosphorus dissolves in some volatile fluids, notably in carbon disulphide, and in this solution can be kept ready for use. The solution was made by dissolving phosphorus in carbon disulphide up to the point of saturation. The fluid was kept in a well-stoppered bottle, was retained, ready for use, in a cold place, and was labelled "the standard phosphorus solution, saturated." If, afterwards, it was considered necessary to lessen the quantity of phosphorus for experiment, more carbon disulphide was added, and the quantity of phosphorus was recalculated. It was, however, rarely necessary to dilute the standard solution. It was best in almost every case to reduce the quantity of solution used, and so save time and trouble in removing the solvent. In all instances the phosphorus was as finely distributed as possible through the substance to be oxidised, the process of admixture being performed in closed vessels, either by agitation, churning, or good trituration at a low temperature.

After complete admixture and distribution of the phosphorus through the substance in combination with its solvent, the solvent was removed, so as to leave the phosphorus free. Two methods were employed for this

object, but sometimes the two modes were combined. One of these plans was that of putting the compound under the receiving jar of a good air-pump and exhausting until all evidence of the escape of the bisulphide vapour ceased to be supplied. There was an advantage in this from the fact that, as the oxygen of the air, contained originally in the bell-jar, was quickly pumped out, the mixture could be preserved from oxidation for any length of time, ready for service when it was required. In other instances I dispensed with the air-pump and used warmth for evaporation of the solvent. The carbon disulphide evaporates rapidly at a temperature of 105° Fahr., and when I wished to oxidise directly, by means of oxidised blood or of solution of oxygen, the liberation of the sulphide vapour proceeded side by side with the absorption of oxygen. Nature does what is analogous to this process in many of her designs, as in the act of respiration, where the carbonic acid is exhaled as the oxygen is absorbed. In certain other experiments I combined exhaustion with a high temperature, in order to secure a quick liberation of the solvent.

For the purpose of regulating warmth and subjecting the specimen experimented on to fixed degrees of temperature for longer or shorter times, I had constructed for me a chamber after the manner of the incubator devised by Messrs. Hearson. The chamber, which measures internally 8 inches by 10 by 10, is made of well-seasoned wood, covering an inner metal chamber filled with water. It is provided with double doors—one door (the outer) of wood, the other (the inner) of glass. The chamber is heated by a gas jet beneath it, and the heat is regulated by the expansion or contraction of a "capsule," which regulates the flame of gas with great nicety. A thermometer entering the chamber by a small opening at the top

or roof registers the heat inside, and the regulation of the heat is so planned that I can work at any temperature between 95° and 110° Fahr. My assistant soon learned the management of the lever connected with the "capsule' so accurately that, having "set the chamber" at any degree within its range, the heat could be maintained without variation for weeks at a time. This has been a great convenience. It has enabled me to digest, if I may so express it, various structures, tissues, and fluids with phosphorus, either with or without oxidation, at a precise temperature, and feel that if I were obliged to leave an experiment, as so often happens in obedience to the calls of professional life, the experiment would continue exactly at the point desired without danger of any failure from change of temperature.

In dealing frequently with the phosphorus solution care has to be taken against accidental combustion from evaporation. For removing the phosphorus fluid during experiments, glass pipettes of different sizes act best. The lower end of these is passed through the neck of the bottle, and is charged direct from the bottle. This prevents the necessity of pouring out the solution, and makes the measurement of quantities very exact. Care also has to be taken, during manipulation, not to inhale the phosphorus vapour. A little phosphorus always goes over with the vapour of the disulphide and causes irritation, with some possible danger of phosphorus necrosis if proper precautions of good ventilation and skilful and clean manipulation be not considered. I name these practical points as guides to other experimenters who may, perchance, follow in my footsteps.

In the way of other means for research I provided myself with oxygen in two forms; one, a bottle of

Brin's compressed oxygen, which is exceedingly convenient; the other, a specimen of oxygen peroxide, the oxygen peroxide prepared in varying strengths by volume.

Attempted Synthesis of Pus by Phosphorus Eremacausis of Nervous Matter.

Referring once again to the essay on the cerebro-spinal axis published in last Asclepiad, and also in the *Journal of Mental Science* for January last, the reader will find recorded that in the oxidation of phosphorus finely distributed through brain substance a slow combustion, a true eremacausis, can be established, with results which suggest the idea of reorganisation of animal material, and with secretion of a specific fluid. The observation led me to try what would be the effect of admixing thoroughly phosphorised white matter of the spinal cord with serum of blood, and then digesting the mixture at a temperature of 100° Fahr. in the presence of oxygen.

To carry out this inquiry, I used the fresh spinal cord of the sheep. The portion of cord operated on was freed of membrane, was rubbed through a fine muslin sieve, and was then treated with the standard phosphorus solution by trituration. The little mass thus produced was placed under the air-pump in a small flask, the mouth of which was filled with a perforated cork, having a light valve over the perforation, so that during the exhaust the vapour of the carbon disulphide could escape without admission of air when the exhaustion ceased, and when the flask was removed from the receiver of the pump. So long as the disulphide was in contact with the brain substance and serum, oxidation was impossible, the presence of the solvent being a sufficient check; but when the solvent was withdrawn the oxidation from the air was so active, if the phosphorised mass were exposed to

air, that fumes of phosphoric acid formed, and the mass was seen to be luminous in the dark when the cloud of fumes was blown away. The light valve at the top of the flask prevented the admission of air, and enabled me to keep oxidation in check until it was time for permitting the slow combustion to proceed.

After the disulphide was quite exhausted the phosphorised mass was treated with a weak solution of hydrogen peroxide rendered alkaline with solution of soda. The flask was then placed in the warm chamber, heated to 100° Fahr., and left to digest for twelve hours, additional oxygen being added from time to time until evidence of phosphoric acid fumes ceased on the further addition of oxygen, and oxygen began to be evolved. The experiment was then considered to have reached the close of its first stage, and the product was ready for examination.

The product removed from the flask consisted of a creamy fluid so nearly resembling what is commonly called "laudable" or healthy pus, it was impossible, on common observation, to know it from that fluid. A specimen of fresh pus placed beside it was so closely the same in appearance, it was necessary to label the two specimens to prevent them being confounded one with the other. From each of them on standing a few hours there exuded a small quantity of sanious fluid, and each deposited a caseous-like layer. But it was in the microscopical appearances that the similarity was most perfect. I failed, in fact, to distinguish any difference between the two specimens. Both presented dead pus corpuscles of the ordinary character, spherical with sharp outlines, fine granular surfaces, and the usual greyish white colour.

COTEMPORARY PRACTICE AND LITERATURE.

"*Every physician will and ought to make observations from his own experience; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together.*"—FRIEND.

BABEL THERAPEUTICS.

IT would be difficult to find a sadder subject for contemplation than the medical and other essays that have been published during the last three months on the treatment of influenza. What has occurred in this way brings back forcibly the dialogue between the squire and the apothecary, recorded by Dr. Moore, and reproduced in the notice of Moore in last ASCLEPIAD. The apothecary affirmed that his class treated influenza by the "*drench*," the surgeons by the "*lancet*," and the physicians "*some of them by one thing and some of them another.*" But the squire treated all his retinue by two remedies, a warm bed and barley-water, or if the patient preferred gruel to barley-water, then by gruel in place of barley-water; and the best of it was that all the patients, under this simple treatment, recovered. Here was plain and reasonable treatment, but the treatment that has been brought into practice in this latest epidemic baffles description. I have calculated over one hundred different plans, each having something peculiar, and many being as wide apart from the others as the poles of treatment can be. Those who hold to the

germ origin of the disease have stood out for the treatment by what are called germicidal remedies. Those who have looked upon it as of the character of a catarrh have held by diaphoretic measures; the homœopaths have decided that their medicines are the true specifics; and the sceptical therapeutists have quietly followed Dr. Moore's squire and gone in for warmth, rest, and barley-water—or some equivalent of barley-water—and have prescribed medicinally a placebo, at most, to satisfy the faith of believers in physic of some kind, of whom there is still a considerable majority in the world at large. These have been the four grand winds of treatment, but within them there have been lesser gales, sharp and furious. The question of stimulation has raised an active controversy, and has led to severer dispute out of the pale of medicine than within it, one enthusiast for brandy as the sheet anchor of treatment, going the length of distributing that unreliable fluid wholesale, and receiving from abstainers the accusation of doing an act positively dangerous and corrupting. For my part, looking on the scene that has been presented, without any prejudice, and simply as an observing learner, I have been forced to a conclusion which may be briefly stated. From the experience of the different lines of treatment that have been brought forward in the examples of the disease which have in this outbreak come under my observation, the great subject of amazement has been the remarkable recoveries that have taken place under diversest plans of treatment. Some patients have recovered under quinine; some under salicine; some under quinine and ammonia; some under salicylate of soda; some under ammonia acetate; and so on without much uniformity of action, even when the same remedies were employed. Forty-five years ago the treatment was much more uniform. It

consisted in the administration of acetate of ammonia, with carbonate of ammonia, and nitrate of potassa; or of effervescing saline mixture, combined with complete rest and low diet. In simple cases this treatment, with the addition of an alterative or purgative, was considered sufficient for the acute stage; a so-called tonic of bark and ammonia, or of quinine and a carminative, coming in, finally, to "give strength" during convalescence. But if a complication in the form of pneumonia or bronchi-pneumonia or pleurisy appeared, then, when the oppression of respiration was considerable, we unhesitatingly took blood from the arm, in order to relieve the embarrassed circulation. In this line of treatment there was consistency, to say the worst of it, a virtue that has been entirely absent at the present time, except amongst the homœopaths, who, as many would think, have been consistent in protecting their patients from open danger, in prescribing placebos, and in making Nature the senior partner in their ministration. All this has led me to the impression that the mainspring of recovery, when it has occurred, has been of the natural order, and that with one exception medicaments have had little to do with the results, either for life or death. Certainly no approach to a scientific mode of cure has been so much as formulated, nothing from which anything like a law of treatment or canon could be constructed for service in the future. The exceptional treatment to which I refer bears on the use of alcohol, and that use, I can conscientiously say, I have seen actually hurtful. In two cases where it had been administered up to the second degree of its action, and where it was leading to serious vascular relaxation, I stopped it straight, with the best results; and in another case where it had been carried up to the third degree of its action, inebriation mistaken for febrile delirium, the

symptoms of fatal catastrophe which supervened were such a distinct mixture of influenza and alcoholic intoxication, it was impossible to declare which of them was the actual destroyer.

If the account rendered above of the imperfection of our knowledge of the treatment of this widespread and often-recurring disease be true, it is surpassingly sad. We ought in this disease to be able to place the stricken in such a condition that they cannot die from it. Moreover, this perfection of our art ought to be as simple and understandable as it should be positive and triumphant. But we stand still in ignorance, pure ignorance, of first principles of cause. We do not know, as yet, whether the first impression of the affection is inflicted on the solids or the fluids, on the nervous organism or on the blood; whether the vascular system is thrown out of gear by a primary nervous derangement, or the nervous by a primary vascular derangement. Until this elementary point is cleared up the Babel of treatment must still prevail, every man speaking his own prescription, and Nature for good or for evil—for Nature goes her own way—reigning as dominant arbiter of life or death.

"*A HISTORY OF EPIDEMICS IN BRITAIN FROM* 664 A.D. *TO THE EXTINCTION OF THE PLAGUE*, 1666."*

[By Charles Creighton, M.A., M.D.]

SINCE the appearance of Hecker's great and classical work on the plagues of the middle ages, no such book as this by Dr. Creighton has been given to the world. In many points of view, indeed, this book surpasses its predecessor. It is

* Cambridge, at the University Press (1891).

more laborious; it goes deeper into origins; and, although it professes to deal with the subject of plagues confined to British soil, it takes in a wider field than any other book or history, bearing on plagues, that has preceded it. If it is less of a story than Hecker's, it is more of a history, and far more of a philosophy. We have all known, and some have jealously known, the author as a scholar; we have all known, and some have uncomfortably known, the author as an independent, bold, truthful, and uncompromising critic of popular medical speculations and practices; but we have not previously, to the full extent, tasted his industry, his antiquarian spirit of research, and his grip of learning. Over the gulf that separates the living and the dead, he might shake hands with Gibbon, if the gulf were spannable. He resembles Gibbon, too, in that his history spares no dogma. It goes straight on, sweeping like a current that acknowledges no hindrance to its commanding course.

The volume is a big one to master. It covers 692 pages, each page containing close upon four hundred words, to say nothing of copious footnotes, and at the end of the volume a very useful index, that has to be grasped before the study of the book is finished properly. Read at the rate of twenty pages a day, which is as much as a practised reader can profitably read, mark, learn, and inwardly digest, the scholar who tackles the book honestly and bravely has five weeks of good study before him, with not a moment too many, either for the labour, the profit, or the pleasure.

The volume opens with a chapter on pestilences, previous to the Black Death pestilences, arising from famines. It describes the plague of 664—684 A.D., recorded by Beda, and its probable relation to the plague in the reign of Justinian. Then, chapter by chapter, he is led to the

study of "Leprosy in Mediæval Britain"; "The Black Death of 1348-9"; "England after the Black Death, and the Epidemics extending to 1485"; "The Sweating Sicknesses of 1485—1551"; "The Plague in the Tudor Period"; "Gaol Fevers, Influenzas, and other Fevers in the Tudor Period"; "The French Pox"; "Small-pox and Measles; Plague, Fever, and Influenza from the Accession of James I. to the Restoration"; "Sicknesses of Early Voyagers and Colonists"; "The Great Plague of London"; and, "The Last Plague in England." This brief outline shows a remarkable roundness and completion of system, which will appear much more distinctly when the volume is read. It relates to a beginning and an ending of an epoch. What may have occurred previously to the reign of Justinian; what may have followed the cessation of plague in England, soon after the Great Plague of London, may remain: but, in the period measured out, there is something compassed that comes logically into discussion, admitting of analysis as to facts and of fair synthesis as to theory. The facts run like a mighty experiment; the theory stands out as a necessary induction. It would be flattery to Dr. Creighton, which he would be the first to resent, if it were said that his theory was as remarkable as his history; and it would be no wonder to have to say of him that, shining as an historian, he was out of his element as a theorist. But an extreme view on either side of this matter would be false; for, in plain truth, his theoretical skill, if inferior to his historical, is far above the ordinary level, and with this advantage: that it is neither tied nor prejudiced by any modern school of dogma, however declaratory and however popular. For example, when he is treating of plague as a disease species, and is obliged to touch on the free and easy modern germ hypothesis, he tells us it is our duty to construct the

best hypothesis we can on history, sparing no labour. "No one really," he says, "dispenses with theory, whatever his protestations to the contrary; those who are the loudest professors of suspended judgment are the most likely to fall victims to some empty verbalism, which hangs loose at both ends, some ill-considered piece of argument, which ignores the historical antecedents and stops short of the concrete conclusions. It has been so in the case of infective diseases, and of bubo plague in particular. The virus of plague, we are told, is specific; it has existed from an unknown antiquity, and has come down in an unbroken succession; we can no more discover how it arose, than we can tell how the first man arose, or the first mollusc, or the first moss or lichen; its species is indeed of the nature of the lowest vegetable organisms." "The objection to that hypothesis," he continues, "is, that it involves a total disregard of facts. It is a mere formula which saves all trouble, dispenses with all historical inquiry, and appears to be adapted equally to popular apprehension and to academic use. The bubo plagues of history have not indeed been all of one descent. Again, the attempt to link in one series the various scattered and circumscribed spots of plague, now or lately existing, must fail disastrously the moment it is seriously attempted. The hypothesis of one single source of the plague, of a species of disease arising we know not how, beginning we know not where, but at all events reproduced by ordinary generation, in an unbroken series of cases, *ab aveo ab ovo*, is the merest verbalism, wanting in reality or concreteness, and dictated by the curious illusion that a species of disease, because it reproduces itself after its kind, must resemble, in other respects, a species of living things."

With the reproduction of this admirable piece of criti-

cism, which the reader who yearns only for what is true will apply to present as well as to past events and ideas, I must leave medical scholars at large the profit and pleasure of drinking from a prime source, to which, as a guide in advance, I have simply directed them.

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*"SATURN'S KINGDOM, OR FABLE AND FACT."**

[By Charles Moore Jessop.]

"JAM *redit et Virgo, redeunt Saturnia regna.*" So Virgil comes, in words, back to us again. The Saturnian kingdoms return. The venture of Mr. Jessop is a bold one. But to say of it that there is in it fable is scarcely correct, since the fable lies, merely, in the name. The kingdom of the fabulous Saturn was one of great glory; and it is true that our present author does indulge in fable when he tries to compass the whole physical and vital history of the planet, as within the happy and mystical *Saturnia regna.* Virgil, in his sublime song, rejoiced that the Saturnian glories would return; Mr. Jessop writes as if they had never ceased to be, the only bit of fabulous lore there is in the whole of his compact little volume of 294 pages.

As a matter of fact, the book is a clever conception of the evolution of things planetary, derived from the labours of human science, as far as the author can read science from a broad, sometimes deep, and always catholic point of view. Haeckel, Croll, and Darwin stand him well as sponsors, selected by himself; and on them he rests mainly for evidence; but he takes in many more, including Helmholtz, Daniell, Thomson, and Tait, and, to the wonder of the sceptical, the great Moses himself.

* Kegan Paul, Trench, Trübner & Co., Limited (1891).

The book is useful reading, and if the writer had but had the courage and literary craft to repeat his nouns, instead of indulging in everlasting pronouns, which toss his meanings, sometimes, into wildest confusion, it would be a delightful volume. Haeckel's pedigrees of life are excellently rendered. "The History of the Egg," "The Animal Kingdom," "The Antiquity of Man," "The Passions and the Intellect," are titles of chapters, which to busy medical philosophers are rich in teaching, and have even a kind of useful bearing on readings of disease. In another chapter on "The Known and the Unknown," in which the writer deals ably with the views of Weismann, there is a ring of sound common sense on the subject of first mental impressions and the training of the young. Mr. Jessop may, indeed, be warmly thanked for an epitome of knowledge bringing together many subjects seeming to lie very wide apart, but which really lie close together, and for leading the mind, in a simple and reverential spirit, to the study of subjects which many shrink from as too obscure and dangerous to be faced without a guide. This object appears to have been his intent, and he has succeeded admirably.

*"THE CHINESE."**

[By Robert Coltman, M.D.]

IN the year 1885 Dr. Coltman, at his own request, received his credentials from the American Presbyterian Board of Foreign Missions to go as a physician, on their staff, to North China, that is to say to that part of the Celestial Empire that lies to the north of Shanghai. He left his American home in March,

* F. A. Davis, Philadelphia and London.

1885, and, taking with him his wife and infant son, went to the post assigned to him, and, after four years' residence there, with many opportunities for visiting various parts of the country and becoming familiar with its people, their manners and customs, he returned to America charged with much important information, some of which, at the instigation of friends, he has supplied in the volume now under notice.

He who wants a pleasant book with which to while away an occasional hour, for a few weeks, should read this book. It introduces civilised Westerns into a new world, and is as fascinating as the later adventures of Robinson Crusoe in the same regions of the earth; indeed, the story told by Defoe of the Chinese gentleman sitting under the shade of his summer residence is more than once vividly recalled to the mind by Dr. Coltman, who mixed with people of all grades in Northern China, and learned their domestic life thoroughly. As in England and America, there are in China three classes of people, the upper ten, the middle class, and the lower class, each as distinct as obtains here; but whether amongst the lower class there is a submerged tenth he does not clearly inform us; we must suppose there is, since criminal classes are not wanting. The community generally is not rich, and the masses lead a life of intense hardship and drudgery. There is abundance of wealth concealed in the soil, but a deficiency of enterprise, whilst an overbearing jealousy amongst the ruling officials stands in the way of advancement. All China lies open as yet to the introduction of the railway system, and when that system is introduced old things will pass away, and everything will become new.

The chapters on "An Interior City" and on "Visiting the Peasantry" are unusually pleasant for perusal; but the one that will please the medical scholar most is on

"Diseases prevalent in China." Diseases of alimentation stand first on the list, and are the most generally distributed. The upper class can obtain what it desires, and lives well; the middle class can enjoy a sufficiency of proper food of a digestible nature; but, by far the largest class of China is the labouring class, who can barely earn enough to secure food of the coarsest description, food containing so little aliment that a large quantity has to be ingested to support life. This leads to dilatation of the stomach and thinning of its walls, with consequent impairment of stomachic function. The effect of such bad aliment is severely felt by the very young, and the mortality of infants in China, if it were known, "would astonish the world." Another factor in the production of disease is the bad water supply, the entire absence of hygiene, and the malarial climate. Amongst acute and spreading diseases, small-pox is the most common in China. Nearly every one has it at some period of his or her life. No region is free from it; it may be called resident everywhere. Vaccination is practised, but the virus is so often a source of danger, the author seems to doubt if the supposed prophylaxis is not as injurious as the disease. Measles, scarlet fever, and diphtheria exist in China, but erysipelas is very rare, and typhoid is comparatively rare. Typhus is frequently met with. Cholera appears, as an epidemic, every few years, and is frightfully fatal. Chronic muscular rheumatism is common all over China, but is unattended by fever. Malarial fevers are reported everywhere; Shanghai reports a pernicious type of remittent fever, with scarcely any remission, that is largely fatal, and is rarely absent.

To put the whole story of disease in China into a nutshell, there are found in that empire all the conditions, less the influence of the Church, that were present in

Europe during the middle ages, and with the same conditions there are the same diseases. As Dr. Creighton describes the past in Europe, Dr. Coltman describes the present in the Celestial Empire.

*"OYSTERS, AND ALL ABOUT THEM."**

[By John R. Philpots, L.R.C.P., J.P.]

ALTHOUGH the title of this work is ambitious, there is reason in the effort, for, surely, never was more matter of fact put together on one subject than is packed in the two full-bodied volumes by Dr. Philpots. The index, "glossarial, explanatory, and referential," makes, of itself, a hundred closely printed pages, and abounds in information. As to the text, turn where we will, there is something curious and useful to be found. Thus, in comparing the nutritive value of the oyster with that of other animal substances, we learn that investigations on Schleswig-Holstein oysters demonstrate that the entire animal contains from 21·5 to 23 per cent. of solid material, whilst the body, without the gills and mantle, contains from 23 to 25 per cent. A comparison is then struck between the oyster and other kinds of animal food, from which it is deduced that the oyster contains as much food substance, as the better sorts of meat used for food, such as beef and fowl; in addition, it is still further distinguished from the greater number of animal foods by its being more easily digested; also the value of an oyster does not depend altogether upon the amount of nourishment which it contains, but likewise upon its delicacy and uniformly fine flavour.

* Two volumes. John Richardson & Co., London and Leicester.

These statements will appear strange to many, but they are well sustained, and are now noticed as some of the various and numerous lessons which are to be found all through the pages of the work, each of its kind practical, instructive, and particularly useful to those who are anxious to have on their library shelves books supplying copious references to the labours of predecessors and compeers who have contributed their share of knowledge to the common stock.

It required a mind of great application and industry, as well as one charged with "love of the subject," to produce such a compendium of oyster lore.

TO CORRESPONDENTS.

In order to prevent the frequent and troublesome mistake of addressing communications to the "Editor," it is necessary to repeat that everything published in the ASCLEPIAD *is exclusively by the Author whose name is on the title-page.*

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The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

February 15th, 1892.

APPENDIX OF ACKNOWLEDGMENTS.

ABBOTT, AND THE SECRETARY OF THE COMMONWEALTH OF MASSACHUSETTS. "Forty-ninth Report of the Births, Marriages, and Deaths of the Commonwealth of Massachusetts." By SAMUEL W. ABBOTT, M.D., and the SECRETARY OF THE COMMONWEALTH. (Public Document.) Boston: Wright & Potter Printing Co. 1891.

AITKEN. "The Life and Works of John Arbuthnot." By GEORGE A. AITKEN. Oxford: At the Clarendon Press. 1892.

ALTHAUS. "The Pathology and Prevention of Influenza." By JULIUS ALTHAUS, M.D., M.R.C.P. London: Longmans, Green & Co. 1892.

ANONYMOUS. "A Settler's Thirty-five Years' Experience in Victoria, Australia, and how £6 8*s*. became £8,000." Melbourne. 1891.

BALCOMB. "Diagrams of the Mouth, Fauces, and Larynx." By J. T. BALCOMB. London: J. T. Balcomb. 1892.

BARR. "The Treatment of Typhus Fever, and Reports of Forty-five Consecutive Cases." By JAMES BARR, M.D. With Introduction by W. T. GAIRDNER, M.D., LL.D. London: H. K. Lewis. 1892. [Reserved for special notice.]

BRAITHWAITE. "The Retrospect of Medicine." A Half-yearly Journal. Edited by JAMES BRAITHWAITE, M.D. London: Simpkin, Marshall, Hamilton, Kent & Co. 1892.

BURET. "Syphilis in Ancient and Prehistoric Times." By F. BURET, M.D. Translated from the French, with Notes, by A. H. OHMANN-DUMESNIL, M.D. Philadelphia and London: F. A. Davis. 1891.

COLLINS AND WILDE. "The Pathology of Ophthalmoplegia." By W. J. COLLINS, M.S., M.D., and L. WILDE, M.D. Reprint. London. 1891.

COLTMAN. "The Chinese, their Present and Future: Medical, Political, and Social." By ROBERT COLTMAN, jun., M.D. Philadelphia and London: F. A. Davis.

CREIGHTON. "A History of Epidemics in Britain from A.D. 664 to the Extinction of the Plague." By CHARLES CREIGHTON, M.A., M.D. Cambridge: University Press. 1891.

CUTLER. "Rules for a Dictionary Catalogue." By CHARLES A. CUTLER (United States Bureau of Education). Washington: Government Printing Office. 1891.

DAVIS. "Consumption: How to Prevent it, and How to Live with it." By N. S. DAVIS, jun., A.M., M.D. Philadelphia and London: F. A. Davis. 1891.

DIXON. "Annual Address before the State Board of Health of Pennsylvania." By PROFESSOR SAMUEL G. DIXON, M.D. 1891.

DUKES. "A Significant Fact in the Diagnosis and Interpretation of the Albuminuria of Adolescents." By CLEMENT DUKES, M.D. Reprint. London. 1891.

ESHNER. "Arterial Capillary Fibrosis." By AUGUSTUS A. ESHNER, M.D. Reprint from Philadelphia County Medical Society. Philadelphia. 1891.

ESHNER. "The Diagnosis of Cerebral Hæmorrhage." By AUGUSTUS A. ESHNER, M.D. Reprint. Philadelphia. 1891.

FYFE. "The Progress of Death in Scotland and her Counties since 1855." (Presidential Address.) By PETER FYFE, M.I.E.S.S. Glasgow: Alex. Macdougall. 1892.

GAIRDNER. "On Certain Arrangements made in the City of Glasgow, 1866, with a View to the Prevention of Epidemic Cholera." By T. W. GAIRDNER, M.D., LL.D. Reprint. Glasgow. 1891.

GANT. "The Lord of Humanity, or the Testimony of Human Consciousness." By FREDERICK JAMES GANT, F.R.C.S. London: Longmans, Green & Co. 1891.

[A second edition of Mr. Gant's most interesting work, with some new matter deserving carrful perusal.]

GOULD. "The Practical Adjustment of Spectacles." By GEORGE M. GOULD, M.D. Reprint. Philadelphia. 1892.

HINDEKOPER. "Age of the Domestic Animals." By RUSH SHIPPEN HINDEKOPER, M.D. Philadelphia and London: F. A. Davis. 1891.

HODGINS. "Aims and Objects of the Toronto Humane Society." Edited by J. GEORGE HODGINS, M.A., LL.D. Toronto: William Briggs. 1888.

JESSOP. "Saturn's Kingdom, or Fable and Fact." By CHARLES MOORE JESSOP. London: Kegan Paul, Trench, Trübner & Co. 1891.

LAWRENCE. "Presidential Address to the Harveian Society." By H. CRIPPS LAWRENCE. London. 1891.

LEEUWENHOEK MICROSCOPICAL CLUB. "A Review of the Work of the Leeuwenhoek Microscopical Club." Manchester. 1891.

LONGMORE. "Richard Wiseman, Surgeon and Sergeant-Surgeon to Charles II." A Biographical Study. By SURGEON-GENERAL SIR T. LONGMORE, C.B., F.R.C.S. London: Longmans, Green & Co. 1891.

MARBLE. "Sanitary Conditions for Schoolhouses." By ALBERT P. MARBLE (Bureau of Education). Washington: Government Printing Office. 1891.

NEWMAN. "Secret Hymns." Edited by EMERITUS PROFESSOR F. W. NEWMAN. Nottingham: Stevenson, Bailey & Smith. 1892.

NISSEN. "A B C of the Swedish System of Educational Gymnastics." By HARTVIG NISSEN. Philadelphia and London: F. A. Davis. 1891.

PHILPOTS. "Oysters, and all about Them." By JOHN R. PHILPOTS, L.R.C.P., J.P. Two Volumes. London and Leicester: J. Richardson & Co. 1891.

RAE. "The National Temperance League's Annual for 1892." Edited by ROBERT RAE. London: National Temperance Publication Depôt. 1892.

RITCHIE. "Brighter South Africa, or Life at the Cape and Natal." By J. EWING RITCHIE ("Christopher Crayon"). London: Fisher Unwin. 1892.

ROSE. "On Harelip and Cleft Palate." By WILLIAM ROSE, M.B., B.S. Lond., F.R.C.S. London: H. K. Lewis. 1891.

SAVILL. "Prescriber's Companion." By THOMAS SAVILL, M.D. London: John Bale & Sons. 1891.

SISWELL. "A Study of Influenza and the Laws of England concerning Infectious Diseases." By RICHARD SISWELL, M.D. Lond., M.R.C.P. London: Longmans, Green & Co. 1892.

STEVEN. "The Pathology of Mediastinal Tumours, with Special Reference to Diagnosis." By JOHN LINDSAY STEVEN, M.D. London: H. K. Lewis. 1892.

STRAHAN. "Instinctive Criminality: its True Character and Rational Treatment." By S. A. K. STRAHAN, M.D. London: John Bale & Sons. 1891.

WELLS. "Recollections of the West London Hospital." A Presidential Address. By CHARLES WELLS, M.D. London: Baillière, Tindall & Cox. 1891.

WHITE. "Promotions and Examinations in Graded Schools." By EMERSON E. WHITE, LL.D. (Bureau of Education). Washington: Government Printing Office. 1891.

WOODBURN. "Higher Education in Indiana." By JAMES ALBERT WOODBURN, Ph.D. (Bureau of Education). Washington: Government Printing Office. 1891.

Exchange list, revised, will appear in next number.

KOPER. "Age of the Domestic A
D. Philadelphia and London: F. A.

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OP. "Saturn's Kingdom, or Fab
London: Kegan Paul, Trench, Trüb

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London: Longmans, Green & Co.

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RITCHIE ("Christopher Crayon").

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London: H. K. Lewis. 1891.

VILL. "Prescriber's Companion
& Sons. 1891.

SWELL. "A Study of Influe
Diseases." By RICHARD SISS
Green & Co. 1892.

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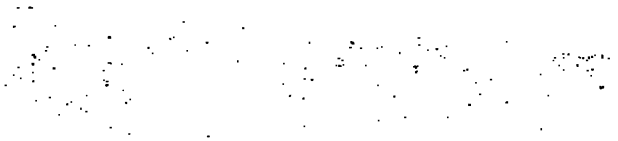
Asclepiad No. 24 Vol IX. Second Quarter, 1892.

Kenelme Digby

Autotype of an engraving by Hollbraken, 1748, after a painting by Vandyke in Kensington Palace.

B.M.



Asclepiad No. 34, Vol. IX. Second Quarter, 1892.

Kenelme Digby

Autotype of an engraving by Houbraken, 1748, after a painting by Vandyke in Kensington Palace. B.M.R.

ON THE CAUSE AND PREVENTION OF DEATH FROM CHLOROFORM.

*LECTURE II.**

MODE OF ADMINISTRATION OF CHLOROFORM AS A POSSIBLE CAUSE OF MORTALITY.

SINCE the introduction of chloroform as a general anæsthetic, great attention has been paid to modes of administration. Arguments have been offered on one side to prove that no special apparatus is required, and that all elaborate apparatus is worse than useless. The first observer of the anæsthetic properties of pure chloroform and the introducer of it into practice —Sir James Simpson—from first to last reasoned against complicated methods of administration. He always maintained that to observe the effect of the chloroform on the system of the patient was the one duty demanded of the administrator, and he naturally had a large and influential following. I have found in my wandering amongst hospitals that in almost every hospital some apparatus has been used for a time and has then been thrown aside as embarrassing and of no real value. There has been, however,

* Delivered March 1st, 1870.

and still is, an opposite school which declares in the most decided voice for an assumed perfect apparatus, and which has supplied instruments of various kinds. Dr. Snow was the energetic founder of this school, and many years of his thoughtful and earnest life were devoted to the elucidation of the one absorbing idea that the dangers of chloroform could be successfully met by the regulation of the quantity of chloroform used by the administrator. So sincere was John Snow, so hopeful, so industrious, I do not wonder he made converts to his way of thinking, and I regret few things more than that I, as one of his converts, should have been driven by force of fact to dissent from his views. Snow's theory was that a given per cent. (from 4 to 5 per cent.) of chloroform vapour in the air inspired was amply sufficient for the production of the deepest anæsthesia, and that more was unnecessary and dangerous. He constructed, therefore, apparatus which should limit the quantity of chloroform to the 4 or 5 per cent. First he constructed what he called the chloroform balloon, which was in fact a bag for receiving chloroform. The description of this bag I will read to you from my edition of his posthumous work published in 1858. The passage is at p. 80 of the work. He there says, "The most exact way in which it is practicable to exhibit chloroform to a patient about to undergo an operation is to introduce a measured quantity into a bag or balloon of known size, then to fill it up by means of the bellows and allow the patient to inhale from it, the expired air being prevented from returning into the balloon by one of the valves of the face-piece to which it is attached. I tried this plan in a few cases in 1849 with so much chloroform in the balloon as produced 4 per cent. of

chloroform in proportion to the air. The effects were extremely uniform, the patients becoming insensible in three or four minutes, according to the greater or lesser freedom of respiration, and the vapour was easily breathed, owing to its being so equally mixed with the air." Snow did not attempt to bring this method of inhalation into general use because he thought it was inconvenient. He sacrificed, he asserts, "absolute perfection to convenience," and thereupon he invented the inhaler which has so long borne his name, and of which there have been many imitations. "Insensibility," he urged, "is not caused so much by giving a dose as by performing a process." And he shrewdly added, "Nature supplies but one mixture of diluted oxygen, from which each creature draws as much as it requires, and so in causing narcotism by inhalation, if a proper mixture of air and vapour is supplied, each patient will gradually inhale the requisite quantity of the latter to cause insensibility according to his size or strength." The argument is forcible, and forcibly put, and it has been employed in various terms many times by other writers. The question remains, Is the argument sound? I fear it fails. It fails because chloroform in vapour is not a fixed substance like oxygen gas, but is derived from a fluid under the influence of heat, and depends upon temperature for its perfect diffusion in even a limited quantity of air. It fails because chloroform vapour after it is condensed in the blood is not applied by the blood as oxygen is, and as far as we know is not changed or combined at all. It fails because it does not take into account the effect of temperature in aiding the diffusion of the vapour into the blood. It fails because it does not take into account the different respiratory capacities of different

individuals nor the rate of respiration. Lastly, it fails because in practice it has not been found to give the safety it promised. True it is that a few who have followed the method advanced by Snow have affirmed they have had no fatal cases. But, unhappily, many others following the same method have had fatal cases, so that we are driven to the conclusion either that his successful followers have had unusual good fortune, or that they have been more skilful in administration, independently of any particular method, than the less successful administrators. I think the most successful have been simply most fortunate.

But the strongest argument of all against the percentage theory is, that the most extreme successes have attended administrations which have been conducted without the assistance of any method for measuring percentage of chloroform. In the seventeen thousand cases I have recorded as occurring before a death was chronicled, the mode of administration was as varied as could well be imagined. Suppose, however, one particular instrument or method had been alone employed in this number of cases, what would have been said of it? It would have been raised to an estimate as priceless as fabulous. In two of the largest hospitals of the United Kingdom there is no record of a death from chloroform, and yet in neither of these institutions has an apparatus for the measurement of chloroform been employed. On the whole, were I inclined to follow up the question sharply, I might urge, on fair evidence, that there have been more deaths from chloroform in cases where an apparatus for adjusting the quantity of chloroform has been used, than in other cases; but this, again, would be false to nature, as implying that proportion of deaths bears

some relation to mode of administration. We will, therefore, pass on our way, content to know that the natural facts dispel the illusion that in any particular known method there is special security from death.

Can we, then, by any reasonable hypothesis, explain, apart from method, the cause of the many and frequent deaths from chloroform? It seems to me that a reasonable hypothesis accounts for some of the facts. The hypothesis is simple: it is that in every case where a person has to be made so dead to the outer world that he neither sees, nor hears, nor thinks, nor feels, but hangs on to the mere thread of involuntary organic function and action, he is dead in every part except in the mere parts which remain true to involuntary organic work, and that as in all persons these necessary parts are not equally sound, therefore some persons succumb. If this be true, it is not on the percentage of chloroform inhaled that either safety or danger in a given number of cases of administration rests, but on the percentage of originally sound or unsound persons who are subjected in a given number of instances to the vapour of chloroform.

Rapid and Slow Administration.

Still there remains one other consideration. It is acknowledged to be necessary for an operation to induce such a degree of insensibility as shall constitute perfect anæsthesia, and it is very near to the exact truth to assume that to produce this degree a certain given quantity of chloroform must enter the body, whether it enter quickly or slowly. We ought to consider, therefore, whether it be safest to introduce what is required quickly or slowly. There is no possible experience on the human subject that can be to us the poorest guide

on the particular question here raised. But on inferior animals we can study the question with precision, and without subjecting them to pain can arrive at a very satisfactory result. I have studied by this method and will put before you the facts under two or three heads.

First, then, I find that a warm-blooded animal subjected suddenly to vapour of chloroform at a temperature over 70° Fahr. will often cease to breathe and to circulate blood at once; it will die as if from sudden apnœal syncope. I think we now know what, in these cases, is the cause of death. Death is not from the absorption of vapour of chloroform into the blood. No; it is from the direct action of the vapour on the nervous system. The stimulation excited by the vapour causes the animal spasmodically to cease to breathe; then follow the apnœa and arrest of the action of the heart.

Dr. Rutherford, in a most able and suggestive paper in the *Journal of Anatomy and Physiology* for May, 1869, "On the Influence of the Vagus on the Vascular System," touches on this subject incidentally, but not less forcibly. He remarks, "If any irritating vapour be brought before the nose of a rabbit, it instantly closes its nostrils and ceases to breathe, often for thirty to forty seconds. Within three seconds after the cessation of respiration, the heart comes almost to a standstill and continues to beat very slowly until respiration be re-established. The arrest of the heart is due to stimulation of the inferior branch of the vagus by the asphyxiated condition of the blood, for the slowing of the heart does not set in until death approach, if the vagi have been previously divided."

Have we in the human subject the analogous mode of death? Distinctly. In Snow's tables of fifty fatal cases, it is recorded that nine of the patients died

within the minute after the commencement of inhalation, and four of these within a few seconds. Various opinions have been adduced to account for such rapid deaths. Some have urged that the deaths are from fear, others that they are from rapid absorption of the chloroform. The explanation seems simple enough on physiological grounds. The patients have died from the direct action of the vapour on the nervous periphery of the breathing surfaces; they spasmodically ceased to breathe, and the vagus being stimulated by the asphyxiated blood, the heart also ceased to circulate blood. In this same way persons have undoubtedly seemed to die under the influence of sudden fear.

The moral of this point is of deep interest, and has at least two meanings. It tells us that chloroform, being a direct irritant, is, in that sense alone, an objectionable narcotic vapour, and it tells us further that it is a bad practice to commence its inhalation too abruptly, or to force on narcotism *rudely* against time. The same observation extends to other agents than chloroform—it extends to all bodies of the same family. Chloride of methyl and bichloride of methylene also act as direct irritants, and are also dangerous when used against time. There is, nevertheless, a practice of producing instant anæsthesia for short operations, so that the patient is made insensible in twenty or thirty seconds.

On the whole it is a bad, and likely to be a fatal, practice to push on narcotism by chloroform against time. In the case of scared women, feeble children, and irritable and resistent persons of both sexes, the danger of administration is always present until the mind is assured if it be faltering, or tranquillised if it be excitable.

Bearing still on rapid death from chloroform is another observation worthy of notice; viz., that if the lungs be suddenly charged with chloroform vapour, there is often induced a quick contraction of the pulmonary arterial vessels. The lungs under these circumstances appear suddenly blanched as snow, and the right heart is instantly paralysed from pressure of the blood which continues to flow into it, but which cannot be forced into the pulmonary circuit. In these cases there may be one or two efforts at respiration even after the action of the heart seems to have ceased; but when the body is laid open and the heart is exposed to the air there is always some after-action of the auricle and ventricle, from absorption of oxygen by the contained blood, and renewal of muscular irritability. There are many cases on record of death in the human subject where the facts point definitely to this mode of death, and which again suggest the impropriety of narcotising by violence of administration.

We want to know, in respect to very rapid death from chloroform, whether the vapour ever acts directly and primarily upon the heart itself through the lungs. In investigating this point I have never been able to satisfy myself that in an animal with a perfectly healthy heart the heart can be arrested in its function by the vapour of chloroform administered through the respiratory surface in any other ways than those described immediately above. At the same time it admits of the readiest demonstration that the heart may be instantaneously arrested in its motion if chloroform be brought into it directly by the blood. To prove this we have to inject with the hypodermic syringe a few minims of chloroform—ten may suffice—into a vein of a full-sized rabbit. Within a second or two there is

death from instant and perfect arrest of the action of the heart. If we now lay open the thorax we find the heart on both sides firmly contracted on firmly clotted blood which fills all the cavities, while the lungs are intensely congested also with clotted blood. If, further, we try the irritability of the muscles with the galvanic current, we find the heart absolutely dead, while all the other muscles of the body respond vigorously to the stimulus, and continue so to respond, it may be, for an hour. In this case the heart received immediately the fatal dose, and the death was due to the contraction excited by the stimulant. We understand here every step of the process. We follow the chloroform into the right side, over with the pulmonary stream into the left auricle and ventricle, and from the left ventricle into the coronary system of the heart itself, there to excite the cardiac muscular spasm which leads to the irremediable death. Still by this experiment we prove only that the heart is fatally influenced when a large quantity of chloroform is thrown into it through the blood, and we leave it still an open question whether the same arrest of the cardiac action will take place when the vapour of chloroform is carried to the heart through the lungs by inhalation. Twenty years ago it was assumed that in nearly every fatal case death was owing to cessation of the heart, and we are indebted to Dr. Sibson for a very acute and admirable suggestion explaining the cause of the suddenness of the death. The heart, said he in effect—for I forget his exact words—first feeds itself with blood by its coronary system of vessels. It receives, therefore, into itself the first impression of every stroke of itself. If the blood with which it is fed be normal, it is first fed by it; if the blood be abnormal, it is first injured by it;

and so when the blood of the left side is charged with chloroform, the heart is the organ primarily influenced by the agent. In inspiring vapour of chloroform, what, then, do we do? We bring the vapour over an immense surface exposed to blood; there it is condensed, there it commingles with the arterialised blood as it is condensed, and with the blood it travels direct through the left cavities into the structure of the heart. The process is much the same as that of direct introduction through a vein, but yet I cannot in any case, in a healthy animal, bring the heart to a standstill in the same way through the lungs. I always find that as the quantity of vapour is increased the introduction of it into the blood is impeded by the direct action exerted upon the pulmonary vessels. In plain matter of fact, I doubt whether it be possible to kill the perfectly healthy heart by the direct action of a poisonous amount of chloroform introduced into its substance through the respiratory surface. But I will not say the same of a heart that is not healthy, nor of a heart in which the nervous mechanism is injured, the muscular substance appearing healthy; for such a heart might possibly succumb under a dose of chloroform small enough to pass through the pulmonary vessels without exciting contraction in them, and small enough not to produce anæsthesia.

A third series of facts, learned from direct experience in narcotising inferior animals, relates to the inquiry whether, if the idea of narcotising at once and, as we may say, by violence, be given up, it is best to begin with gentleness and to continue slowly, or to begin gently, and, having subdued the preliminary excitement, to push on the inhalation more rapidly. Some administrators give the vapour so cautiously that they rarely have the patient ready for the surgeon under six or seven

minutes. Others carry on the administration quickly through the second degree, and bring the patient into the third in from four to five minutes. Snow's rule was two minutes for infants, three for children, and four or five for adults. The rule is a sound one, as is another rule from the same author: that when the second stage of excitement is vehement it is best subdued by carrying on the inhalation firmly and resolutely. In animals I find that when the first stage is over it is always safer practice to narcotise quickly to the third stage than to go on slowly saturating the body with the vapour, and rendering any accident which might be casual fatal. For the fact cannot be too strongly impressed on slow and overcautious administrators that when an animal body is being charged for a long time with chloroform the whole of the substance introduced is not utilised in producing anæsthesia. Some of it is eliminated into the alimentary canal; some is stored up in the liver, some in the cellular tissue, some in the muscular fibre, in all of which parts it is known that we can detect it chemically. *Excess, therefore, is so much dangerous excess; for if any organ fail while the excess is present the act of withdrawing the chloroform from the breathing is of no avail, since there is a steady reabsorption into the circulation from the body, and the narcotic effect is sustained.* Some perfect illustrations of the influence of moderately short as compared with long administrations have occurred in my experiments for the resuscitation of animals that have ceased to breathe under chloroform vapour, and I can determine, by method of administration, whether an animal shall be recoverable or not after respiration has ceased. If the animal shall have ceased to breathe within four minutes I can usually, in five cases out of six, resuscitate at a temperature between

50° and 60° F. But if an animal breathe even less vapour, for seven minutes, before respiration ceases, I cannot ensure recovery in more cases than one in six.

The lesson taught by this series of facts is, never to induce slow narcosis through every stage, but having carefully felt the way in the first minute or two of administration, to push quickly into the third degree, in which, if the body be not surcharged with chloroform, the danger is comparatively small.

Impure Chloroform as a Possible Cause of Death in Administration.

It has been assumed that danger in the administration of chloroform may have arisen from faults in the quality of the chloroform employed, and that death may have been produced by the impurity of the narcotic. Snow, in treating on this important subject (*Chloroform and Other Anæsthetics*, p. 32), says, "I am not aware of serious consequences having arisen anywhere from the impurity or adulteration of chloroform." In other passages, however, he tells us that when chloroform becomes decomposed from any cause it acquires a greenish-yellow colour, and gives off chlorine and hydrochloric acid; it may also contain alcohol, which some have supposed gives to it caustic properties and makes its vapour irritating; but he never saw any evidence of this, although he had hundreds of opportunities of witnessing the action of chloroform mixed with spirit. He refers to a case in the London Hospital where cough and a feeling of suffocation were caused by the hydrochloric acid with which the chloroform was contaminated; but the inhalation was discontinued, and no ill consequences resulted.

For my own part, I have never but once seen any important source of danger from impurity of chloroform. I was on one occasion about to narcotise a man for a capital operation, and on commencing the inhalation I found considerable irritation produced by the vapour without any signs of narcotic action. After a little time I placed the inhaler to my own nostrils, and then detected clearly an acid vapour and faint evidence of the presence of chlorine. Of course I desisted at once, and sent for a new specimen of chloroform, with which the anæsthesia was carried out successfully. The faulty specimen of chloroform here referred to had decomposed and was giving out free chlorine; but it would be impossible to mistake such a specimen, or to continue to exhibit it without detecting the danger in good time. It was evidently a rare accident, for I had never met with it before, nor have I ever met with it since. Moreover, several times after fatal accidents from the administration of chloroform, specimens from the store of the fluid, under which the accident took place, have been sent me for examination, but I never was able to detect, after the closest examination, any adulteration or other fault.

Under these circumstances I feel sure that faulty chloroform as a cause of mortality must be an occurrence of the rarest order, and I doubt if there be on record one unmistakable instance of a fatal catastrophe from this cause.

Commentary, 1892.

To what was said in the above lecture a few notes bearing on the experience of twenty-four more years may now be introduced. The experience suggests to me nothing that need be taken away, but something

that may be re-enforced and something that may be added. I would specially re-enforce the points relating to the immediately rapid administration of chloroform and the danger of that practice.

I.

I have closely observed the condition of persons who are most susceptible to danger, and have pointed out several times that there is a large number of persons who may be designated *the morituri*, persons ready to die at any moment from very slight disturbing causes affecting their nervous balance and nervous organisation. To proceed to administer to these the vapour of chloroform hastily is to cause a sudden reflex easily made fatal. By some curious forecast, which many call fear, and which, in fact, is a natural fear and hesitation, the *morituri* are often, without reason as it seems to the bystander, conscious, as by a premonition, of facing a danger that may prove fatal. In several instances they have expressed this alarm, and in one painful case with which I am acquainted the patient, who to the very last opposed administration, was in the end overpersuaded to commence inhalation, and was dead before a drachm of the narcotic had been inhaled, the "sharpness" of the vapour appearing to excite a spasm of the chest under which the heart ceased to beat. This was a death from syncope of the lesser circulation, syncope caused partly by nervous fear and partly by nervous reflex and shock.

II.

It is right to repeat the cause of the danger arising from morbid feebleness of, or mechanical difficulty in, the heart. It will be recalled that I found, by experiment, how practically difficult it is to stop the action of the

heart of a healthy and strong animal before the respiration has completely stopped, and that the heart will go on beating under chloroform after the respiration has completely ceased. It was in such cases as these I succeeded so readily in restoring to life, by artificial respiration, animals that were, to all appearance, actually dead. From such observations an inference has been drawn that death under chloroform cannot be induced by a direct effect upon the heart. The error is distinct, and is committed with little consideration. A healthy heart cannot be killed directly by chloroform. So much may be granted. In proper language of thought, a healthy heart cannot receive through the lungs sufficient chloroform to kill it when it is in its full activity. But a feeble heart, or a mechanically obstructed heart, or a heart enervated, irregular in its motion, or tremulous from anxiety or fear, is in quite a different condition. A very small amount of chloroform entering into such a heart may be sufficient to stop its beat, or a very slight obstruction to its pulmonary current of blood may be sufficient to paralyse the right side and to break a column of blood which, once broken, is probably never reunited. The pulmonary circuit of vessels left for a few seconds empty of blood is filled with gaseous matter, against which even an active contraction of the right ventricle has no power to force on a charge of blood. I found this to be the fact in the dead animal immediately after death; I failed to drive over a column of fluid by the pressure of the syringe at a degree much more decisive than could ever be expected from the contraction of the right ventricle. The pulmonary artery, with the nozzle of the syringe through its orifice and firmly tied, would expand under the pressure largely, and during the expansion, however gently induced,

would sometimes rupture; but no pressure would force the fluid, without injury, over the pulmonic circuit from the right into the left heart when once the continuous stream was broken.

III.

It is well to re-enforce the lesson of the risk of prolonging the administration of chloroform, after the dangers of the first degree are over, so as to lead to storage of chloroform in the tissues with after-reabsorption of it into the circulation from the various organs in which it is retained. Chloroform in vapour condenses in the blood, and so condensed is excreted into the permeable tissues, the alimentary canal, and the cellular structure generally. There is no evidence that it undergoes any elementary decomposition within the body; consequently when it is stored up largely it continues to act after it ceases to be required. Hence the cause of danger in those whose tissues are diseased, and the danger of long-continued administration after the immediate risk of breach of continuity in the circulation, incident to the first stage, has been safely passed.

*THE PHYSIOLOGY OF DREAMS.**

"We are such stuff
As dreams are made of, and our little life
Is rounded with a sleep."

IF we take the word "stuff" as meaning our bodies living and moving in what we consider the activity of consciousness, and if we consider sleep as resembling the infinite repose of the space through which we are being carried by the planet, then truly the great poet is right to the letter. Every one of us is dreaming now. Between that which we are now doing and that which we are doing when we are said to dream in sleep there is this simple difference only, if it be a difference, that in the present or wakeful state the will directs or moves with the phantasy, and that in the dream of sleep the will is passive, neither suggesting nor directing the course of the story.

There is a romance about dreams in sleep; and whoever—poet, novelist, historian, philosopher—would investigate the influence of dreams of sleep on dreams of wakefulness would find subject matter for a life of labour. Some of the mightiest events in the whole of human life, some of the most persistent, have had their origin in the shortest of involuntary, sleeping dreams.

* Lecture delivered at the Royal Institution of Great Britain, April 29th, 1892.

Men do not seem to will dreams; dreams come without will, that is to say without being willed consciously, therefore they are dreams; but having come, they do not necessarily pass away when the conscious will returns into activity. Some dreams do—and this is fortunate—but all do not, and it is because some come without being summoned and remain afterwards that they are a mystery. For this reason they were ghostly messengers to the larger part of mankind in days of simple life, when science had no presence nor explanatory reason. Why should dreams come if they are neither wanted, expected, nor called? They must be provoked by messengers unseen and to the world at large unknown. Little wonder that they should have played their marvellous parts, and that, to fervid natures, immortal spirits, gods themselves, should have, as it has been assumed, spoken to men in dreams, when gods and spirits, formulated by the minds of men, were accepted as firmly by belief as if they were veritable fact; as if believing and knowing were one and the same thing, which practically they are in an immense number of matters, even in a modern community that boasts the possession of the Royal Institution itself!

Great men, as well as little, have put their interpretations on dreams according to their cherished beliefs. Hannibal the mighty, with his one eye, wished to steal —I am bound in honesty to use the unvarnished term "wished to steal"—from one of the temples of Juno a pillar of gold which he had drilled into and found to be the metal unalloyed. Thereupon Juno appeared to Hannibal in a dream, and like a virago—she had the credit of being of that stamp even to Jupiter—threatened Hannibal that if he dared to take away the pillar of gold from her temple she would have her reprisal;

she would take good care to have his remaining eye removed from the temple of his brain. Juno had the further credit, founded on belief, of always keeping her promises; so Hannibal, a wise and discreet man, did the correct act when he woke out of his dream. He was a great general, a brave general, and a filching general, loving spoil; but his belief in Juno was equal to knowledge, and he was afraid of her, notwithstanding the weakness of her sex. Instead therefore of bearing away the pillar of gold, he ordered it to remain; and that not so much as a single grain of suspicion should rest on him, he had the dust he had drilled out made into an ornament—some say a ring, but I should rather say a calf (bucalam)—and left it on the top of the pillar, where it will no doubt still be found should the pillar ever be turned up by some enthusiastic antiquarian.

DREAMS AS VIBRATIONS.

I have, I confess, a strong dislike to say a word that shall break through any divine enchantment. It seems such a letting down of man from heaven to earth, such a transformation of him from the spiritual to the mechanical, to teach that dreams after all may be nothing more than the common vibrations of terrestrial media acting upon a corporeal vibratorium. Yet so, it is to be feared, the fact stands. There is a string or a wire in tension on some instrument of music. If we were to strike that wire, it would give us a sound which could be determined by our will into a living or rational sound, an extension of ourselves, consciously extended. One might, if one had the skill of a man like Paganini, make that string discourse in

beautiful language. We cease to strike it and cease to hear a sound. But the wire has not ceased. We bring our friend Professor Hughes's magical microphone into use, and we find the note we had ceased to hear still, for a time, yielding a continuous sound; a dream on the part of the instrument. All musical instruments dream after we cease to play on them.

We may do something more with the tense string: we may pass over it a current or breath of air. If the breath be sufficient, the instrument speaks out like a thing of life; if it be not sufficient, the instrument dreams; and if we bring the microphone into use, we hear the dream so long as the gentlest vibration is sustained.

If in some part of this room we should make an instrument emit a sound which was in harmony with the sound which another instrument would give out, we should set the second into sympathetic vibration, and after we had ceased to hear the sympathetic note by our ordinary sense of hearing, we should be able by the microphone to hear it, for again the instrument would dream. Or, once more, if we were to put into vibration other strings, we should bring out a series of discordant noises, confused, unpleasant, and audible by the microphone, even when we did not appreciate them directly, or, if I may so say, by the naked sense.

In these similes I have cast a glance at certain vibratory movements which occur and are the causes of some dreams in men, women, and animals. Dreams are vibrations varying from those of the waking dream down to the most delicate and accidental. The sleep that is free from dreams is therefore that which is most clear of any vibratory movement derived either

from within the body or from without. Some physiologists have contended that no sleep is quite free from dream, but that many dreams are existent that make no impression on the memory, and are therefore as if they had no existence. Whether this hypothesis be sound or unsound is difficult to say, for although it be true that the memory carries dreams that are so intense as to be like waking dreams, it is also true that the body often wakes with weariness for which there is no accounting except the weariness of dreamings which have left no impression on the memory. Again, acts are sometimes performed in sleep which are not carried in memory on becoming awake—a fact which indicates that active dreaming may be present and leave no story.

The most perfect sleep, the sleep least molested with dreams, is one in which the sleeper sinks into repose and wakes again after several hours, having no more consciousness of lost time than if he had closed his eyes and opened them once more. This is what Wordsworth called "the twinkling of oblivion," and a healthful twinkling it is. It belongs perhaps always to the happiest days of childhood. In a condition of body leading to such an elysium, the balance of all the organic parts is being accurately timed and sustained. There is no jarring chord within the organism to disturb by its own motion; the senses rest too soundly to allow of the conveyance of any vibration from without. If then there be a dream, it must be of the lightest kind, so light that not even the responsive movement of a limb is perceivable. In adult life such sleep is exceptional, and marks out an exceptionally healthy and strong individual, in whom mental placidity balances physical strength, one who even during waking hours is less

perturbed than others about him by the clamours of life. I know an individual of this kind who can lay himself down in a Pullman car and sleep from London to Glasgow without being conscious of the journey and without recalling a dream on the awakening—the nearest perfection, perchance, of adult repose in nervous serenity. Such persons are to be envied; they never grow prematurely old; and when they are stricken with disease of a passing kind, they are led to believe in the doctrine Menander taught, that sleep is the natural cure for all diseases.

There are not many of this dreamless nature; the majority of men and women dream, and some so intently that they work, it may almost be said, as earnestly by night as by day, waking in fact as fatigued as when they resigned themselves to rest. A limited few acquire so much the habit of dreaming, they fail to distinguish between dreaminess and wakefulness. They call themselves insomniacs, declare they never sleep at all, and wonder why it is that after they have risen from what they believe to be a perfectly sleepless night they are able next day, improving hour by hour as the day progresses, to carry out their ordinary labours and duties with fair activity; as if, without knowing it, they had slept, after all, in some part of their nervous organisation.

PHENOMENA OF DREAMS.

In referring to these different classes of sleepers and dreamers, I am bordering on the topic of causes of the phenomena of dreams, about which it will be necessary to speak at some length. But it will be most philosophical, before noticing causes, to dwell upon the

phenomena themselves, the understanding of phenomena being ever the true preliminary method of divining the cause or causes of them. A grand field opens to me here. I might, for lecture upon lecture, stand in this place reporting stories of dreams, stories written or told by the dreamers themselves. Unfortunately recorded dreams thought out and written out by their authors are too suspicious, as a general rule, to be accepted as reliable authority. The most vivid dreams and the most circumstantial are often the most fleeting, and when the dream seems to be borne in the memory, recollection, which is not the same as memory, fails to supply the true account. The recollection of one memory commingles with other memories, and the general tendency is to put together a mixture of dream and fancy. Paul Richter in his narrative of a dream of the universe, the most magnificent story of its kind ever told, affords illustration of this fact. He had been thinking, he says, of the mighty space of the universe, until, lost in the immensity of the contemplation, he fell asleep and dreamed of an angel coming to him who, ordering him to be stripped of his robes of flesh and divested of his gravitating body, led him through the unfathomable abysses of space until he cried, "Insufferable is the glory of God. Let me lie down and hide myself from the infinite. Angel, I can bear it no longer." But this is not the narrative of a simple dream of sleep; it is the dream of a poet touched with the most refined delicacy; a picture drawn with unmeasured care; conceived as celestial, and beyond the grasp of any but the imaginative scholar of natural science, who can make a new heaven and a new earth out of the little sphere in which he lives; it is the dream of a constructive Ovid, singing a new and immortal song.

CLASSIFICATION OF DREAMS.

The phenomena of dreams I have to present, though they lack all fancy and are the most commonplace fact, have this advantage: that they are scientifically valid. Throwing aside for my work of observation all fancy, I determined, nearly half a century ago, to make notes of such phenomena of dreams as should in my professional life be told to me while yet the phenomena were fresh in the minds of those who experienced them. When I had, in this manner, collected a goodly number of phenomena, I began to classify them, and in the end I have come to a classification which, although it may be imperfect, is perhaps more nearly perfect than any that has gone before or been recorded in physiological literature. The classification, simple and easily remembered, is set forth under three heads:—

1. There is a dream which is the effect of an external vibration acting upon the sleeper. In this dream there is always an outside object telling upon the sensorium. We might call it the dream of Queen Mab, in which, as you remember, the sleeper is always assailed by the busy Fairy tickling "the parson's nose as he lies asleep," or some other sense of some other sleeper. But it is better to describe this as the *objective* dream, the dream in which an external movement or thing excites the phenomena.

2. There is a dream in which, without provocation of an external kind, something progressing in the body of the sleeper himself provokes the phenomena. This I call the *subjective* dream, because it is dependent on changes in the subject who dreams.

3. There is the *compound* dream, in which the phenomena are lighted up partly by external vibrations

conveyed to the sleeper, and partly by conditions belonging to the sleeper himself which favour the external interference or exalt its influence.

THE OBJECTIVE DREAM.

The objective dream is not uncommon, and is, as a rule, most distinctive. It is a dream produced by vibration started from the outside of the body. It is a dream through a sound or through an impression on a sensitive open surface. It is never, I believe, through the sight, a fact probably due to the circumstance that the closed eyelids shut out impressions of sight. This, however, is not all, for I once knew a person who slept with his eyes open, and, although he dreamed, he never dreamed as if he had been led to dream from the sight of an object. I have seen, also, a somnambulist with the eyes wide open, but I could never find afterwards that she had seen any object during the sleep; and I am sure it is a mistake to assume that, guided by sight, somnambulists pick their course over obstacles that lie in the way. They put out their hands as if to touch, but they neither see nor hear in the sleep, although they sometimes perform, automatically, feats which convey the idea that their faculties are awake and on the alert.

We are all more or less familiar with the phenomena of the objective dream. Some experience it from slight causes, and in nearly all the physiological experiments conducted to ascertain the time and duration of dreams the action of an excitement on the sensitive surface of the skin, or on the sense of smell, or on the sense of hearing, has been traceable. Thus the effect of pressure on the body produced by weights, or of pressure on the limbs from the weight of the body itself

during sleep, has been found to induce dreams of struggle and wrestle, as if for liberty. The presence of odours has produced excitement, sometimes of the most pleasant, sometimes of the most terrible, dream. A friend of mine who was sleeping in a strange house woke one night in a state of actual exhaustion from the deathly struggle of a dream, in which he thought that he was overpowered by birds of prey. His dream carried him to the gardens of the dead in India; and, what was more strange, it carried him to the story of Philip Quail,—a hero of the Robinson Crusoe type,—a story he had not read for many years. He thought the dream must have resulted from indigestion; but it recurred night after night, and at last he began to suspect, from a kind of reasoning that took place in the dream itself, that the down pillow in which his head was half buried was connected with the cause. He inquired into the matter, and at once discovered that the pillow was stuffed with feathers undergoing decomposition and giving forth a peculiar odour so soon as the warmth of his body produced diffusion of the odour. The pillow changed, he was no longer troubled with the dream. All the details of the mystery were also unravelled. The fact of the odour was disclosed, and the story of Philip Quail, from his recollection of it, supplied a similar and suggestive incident, in that a pillow stuffed with the decomposing feathers of wild birds induced, in Philip, a similar dream.

Sounds, particularly when they are suddenly brought to bear on the sleeper, produce the strangest dreams. In my early days of professional life the sound of the night-bell, familiar as I was with it, always produced a vivid, but necessarily brief, dream—for I was up in a minute—that varied with the manner of the

messenger who rang the bell. A violent clang led once to the idea of a fall from a height, with noise of thunder, as if I were coming down on an avalanche; a series of gentle ringings led me back to school-days, when, at the close of term, I took part with my companions in a peal on the bells of the village church, and that so vividly, I seemed to have the rope of the tenor bell still in my hand.

In these objective dreams, expectation and long watching play an important part; the briefest dream then may become the intensest and most real. A few seconds of sleep—nine winks, literally—will come over the wearied watcher, and afford, from the effect of some outer impression, a dream of long duration, with events occurring in it of the most striking nature. Such dreams have deceived many a sleeper, and have led him, unconscious of the transient "twinkle of oblivion," to give to the world tales of manifestations made to himself that have not merely sounded like miracles, but have acted like miracles in their after-influence. The Hegira itself might count amongst dreams of this nature.

In these brief objective dreams it would seem as if one or more of the senses may be awake whilst the others sleep. My learned and finely observant friend Dr. Hack Tuke dreamt that a gentleman called at his house and stood at the front door. He saw the gentleman from the window, and as no one let him in, he (the dreamer) went to the top of the kitchen stairs, where there was a bell, which he shook forcibly; the bell moved, and the tongue of it moved, but no sound proceeded from it. The visual centre was dreaming; that of hearing was dead asleep.

THE SUBJECTIVE DREAM.

The subjective dream, the result of some vibration within the body of the sleeper, is the dream of indigestion, of pain, of fever, of self-investigation or of self-contention.

The dream of indigestion is a dream always of trouble, of fear, of anxiety, of depression. It is, occasionally, attended by the distressing phenomenon of nightmare, *incubus*, with its difficult breathing, sense of weight on the chest, and palpitation, intermittency, or temporary stoppage of the circulation, a feeling of some terrible blackness or thunder-cloud overshadowing life, a coldness of body, and an awakening in a severe alarm, as if death itself were impending and inevitable. The disturbed dreams of childhood are often of this alarming nature, and many a peculiarity of character, many a superstitious fear of later life, is implanted into sensitive natures by frequently repeated dreams of this order, and especially when, at the early term of life, the surroundings each day are of sombre cast, leading during waking hours to gloomy thoughts, forebodings, and strained contritions for trifling and imaginary offences, against morals, doctrines, or habits, that breed mental doubts and terrors. These subjective dreams are mental discords, disturbances between the voluntary and involuntary nervous systems, which, extending by reflected vibrations from one centre to another, bring all the centres of sensation during the dream into confusion and clamour, until perfect consciousness, with return to common life, is restored.

The subjective dream of pain differs from the dream of disturbance. The dream of pain is the continuance

of pain through sleep. It differs according to the character of the pain. After burns, in which vibration at the seat of injury is most intense and continuous, and during which sleep, if it be not artificially induced, comes on from sheer weariness, the dream is literally one of fire and flame. The dream of toothache or of neuralgia is of accident, of struggle attended with a fall, a crush, a blow, or a stab.

Fever leads to a variable dream—variable, I believe, according to the varying degrees of febrile heat. The active dream, when fever is high, is one of rambling exaltation of mind, followed by depression when fever is low, like the flow and ebb of a tide, from the sensorial organs being, from time to time, at different tensions. The fever dream is attended by sensations which extend through the whole of the body, as if the common sensibility were appreciable by touch, with concentration of the sensibility in particular parts. I remember this experience in the course of a nervous remittent to which I was subjected in early life, and I have often heard repetition of the sensation from others. The most remarkable illustration of the kind is one I have published in the ASCLEPIAD for 1884, pages 294-5, and which will be found in the library. In this instance a most intelligent observer suffering from enteric fever reported to me the recollection of his dreams so soon as he had sufficiently recovered to undertake the task. His dreams were, to those who observed him, deliriums in sleep; but to him it seemed that his brain was passive to everything external. He was aware that any voluntary activity would be at the expense of his vital centres, on which he depended for life; whilst, at the same time, there was an intelligent connection going on between those parts,

so that each could feel what the other was thinking of. After long trouble in this manner, he suddenly woke up in a state of profuse perspiration, and with that the dreams passed away. He was exhausted in the extremest degree, but he felt, as he expressively and graphically described it, a "tingling feeling of life," whereupon he knew he had taken a turn and believed he would live through the disease.

Amongst these subjective dreams there is one which accompanies extremest exhaustion: a state when the body lies between life and death, and when the consciousness of all worldly things has faded away so completely that neither words nor acts nor persons are any longer recognisable, but when movements, sights, and sounds are translated in exaltation perhaps through every sense, and certainly through the senses of sight and hearing. I had occasion once to see a gentleman in this condition of temporary death produced by a mechanical arrest of the circulation of the blood in the outgoing current of blood from the heart. Nothing except the dislodgment of the obstructing cause could save life, and hope of this relief was practically given up. The condition was that of a person rapidly passing away in a struggling dream. By a change—I had almost said an accident—which would not occur once in a thousand instances of this nature, the cause of obstruction did suddenly become loosened and carried away, with almost instant return of consciousness, followed ultimately by complete recovery to a life afterwards prolonged, in fair health, for twenty-two years. From the lips of this gentleman, whilst yet his memory was fresh, I took down the particulars of his dream. All the world had faded from him, and he had not the least knowledge that I had visited him in his sleep and

had endeavoured to rouse him to consciousness. He was by profession a farmer, a fact which accounts for much of what he experienced through a long and terrible dream, in which he had been struggling with all kinds of imaginary foes, human and animal: with burglars, horses, and bulls, that got away from him after he had conquered them, allowed him to go to sleep again, and then returned to the contest. These were the causes of the struggles we had witnessed as he lay in the sinking sleep so near to actual dissolution. I asked him how he struggled, and he repeated to the letter the struggles we had witnessed while he was unconscious.

There is a subjective dream occurring mostly amongst dyspeptics, and which may be considered a dream of regret or of despair. It is a melancholic dream, in which, in the most mysterious manner, conscience, as the dreamer may explain, seems to whisper some strong and yet unintelligible message, or in which some indescribable doubt is conjured up, with so much effect that when the dreamer first awakes he feels as if an insuperable difficulty, which quickly clears away, lay straight before him. This is the dream of apprehension, and is nothing more than an attack during sleep which visits many dyspeptic persons in their waking hours, and which seems to suggest the idea of panic, of an impending anxiety or danger, for the occurrence of which there is no conceivable reason. Allied to the same dream is another of desire to escape from an imaginary pain, peril, or pressure, a desire well expressed in the despairing words, "Oh that I had the wings of a dove! for then would I fly away and be at rest." I knew one subject of this dream who was often heard to mutter portions of these words while

asleep, and who frequently woke with them on his lips, saying them audibly to himself.

Lastly, under this head of subjective dreaming, there is what may be designated the dream of contention, in which, in Pauline paraphrasing from philosophy, the flesh seems to war against the spirit, and the spirit against the flesh, so that the dreamer is bound in bonds, unable to do the things that he would, a very mischievous dream in the history of human action. Some dreams have counterparts, but the dream of contention has none. The most conceited dreamer living rarely, if ever, awakes from a dream assured by it of his own self-importance, a discount on dreaming which is good all round.

THE COMPOUND DREAM.

The compound dream, in which subjective phenomena combine with external impressions, is the most common dream. The impression is often made during the period of dreaming, but it may be made before going to sleep, or it may be revived in the period of sleep, and it may take part in the phenomena of the dream. Vibration of motion is in this manner conveyed. After being on the sea for a time, the rocking or vibration of the vessel may be communicated to the body so strongly that afterwards, during sleep on land, the bed seems to take the motion of the vessel, and the sleeper wakes from a realistic dream astonished to find himself once more on *terra firma*. This is a commonplace illustration, but many more are at hand, one of which I will relate, because it bears particularly on a physiological point at which I shall arrive at a later stage. In 1884 Mr. Edward Payson Weston, the pedestrian, put himself under the feat of

walking five thousand miles in one hundred days, being fifty miles per day. He was so finely trained that on the last day of the trial he walked from Brighton to London without once sitting down or ceasing to travel. He placed himself the while under scientific observation, in which I was allowed to take part; and one of the questions I kept in view was the dreams he experienced after walks in all weathers and on all kinds of roads from the beginning to the end of his task. In all the time he could recall no dream; in fact, he fell asleep at once, and woke from dreamless sleep, one of the decisive aids in the accomplishment of his success. But—and here comes the fact I wish to emphasise—in a later feat he did the walking in a circle, on a path in which there were many laps to a mile. Then dreaming began. As he sank into repose those laps recurred in a painful form, as if he were making somersaults. He would wake up under this dream, and through all the ordeal experienced the same interruption. One day he told me that he could not get it out of his mind that on the previous night he really did make a somersault, and that although he knew it was impossible, because he neither got out of bed nor raised himself, and because he felt sure that the bedstead was not likely to play a trick of the sort, yet nothing could clear his mind from the sensation that he had veritably turned a forward somersault, not once, but several times before he awoke to consciousness. More striking still, he felt, after he awoke to consciousness, that same muscular strain and momentary fatigue which would follow exercise of the kind named. In this instance the impression was, so to speak, set in the body before going to sleep; then came a dream which would, ordinarily,

have been forgotten, but in the course of which the impression was liberated, perhaps, nay probably, by a muscular movement of the body—raising the head on the pillow, for instance—and the phenomena of the somersault were developed.

The compound dream is frequently a continuation of a waking dream. We go to sleep with something on the mind, and either the mental labour continues, or, after a short rest, the subject of contemplation returns and is, or seems to be, sustained until awakening. Two very different lines of mental work are thus sustained: the purest reasoning, the mathematical, and the purest imaginative. I knew a scholar who after a dreary night, in which he slept well physically, woke in the morning with different and difficult problems quite solved, but mentally worn out and ready only for some active sport. I know another scholar who, engaged in a work of imagination, and thinking himself to sleep at the junction of cross roads of thought, has often risen in the morning well advanced on one of them, and inspired to write, as fast as his pen would let him, a passage or a poem.

The compound dream is often one of memory. The memory may be of things recent, but more frequently it is of things, events, or persons, remembered from long ago, but impressed and fixed in the mind. Dreams thus become, as it were, reminders of the past. I inquired of a blind person who had an extensive knowledge of persons in his own unhappy condition whether he or they ever had dreams of visible things. He explained to me that he had, and that others like himself had, but that they always were dreams of objects with which they had become familiar before the loss of the sense of sight. These are dreams of memory

purely, dreams called up by current circumstances that have led to trains of thought connected with the condition of former life when sight aided comprehension. I notice that the prince of physiological writers, Muller, relates of the distinguished Huber, who had been blind from his eighteenth year, that in his sixty-sixth year he still dreamed of objects which appeared distinctly visible to him, though these dreams referred to the time at which he was possessed of vision. In a similar manner the deaf occasionally dream of sounds which they heard before the period when deafness was established in them; and I remember an instance in which a child who was dumb, in consequence of his becoming quite deaf in his second year, would call out words in his dreams, which he never pronounced when awake.

The compound dream is now and then one of suspicion which may attach itself to some particular person who is being dreamed about. This, I have no doubt, is the true explanation of the dream of a woman that Corder was the culprit in the case of the murder of Maria Martin, the victim of what was called the "Red Barn Murder." Here a process of reasoning goes on in the dream, in the course of which many circumstances combine, as in the construction of circumstantial evidence. Such dreams have been considered as revelations, and have been received as manifestations of special character and importance.

Again, the compound dream may be one of hope or of fear, and, according to its nature in these respects, may lead to conclusions which seem like predictions, but which are nothing more than reasonings from possible or probable data. Once in some thousands of such instances the conclusion drawn may in some

degree prove correct, just as it might if it had been arrived at in hours of wakefulness; and, thereupon, the importance of the dream has been magnified to the last degree.

Amongst the class of compound dreams, we have to take in all those which are developed when sleep has been artificially induced by the action of the narcotic series of chemical bodies, such as opium, Indian hemp, mandragora, chloroform, chloral, methylene, amylene, the ethers, carbonic acid, mercaptan, alcohol, coal gas, and other narcotising substances. I wrote some years ago a paper on this topic, in which I showed that each distinctive substance produces its own peculiar dream, probably from the effect it has over the action of the heart, as well as from the direct effect exerted on the sensorium. The subject is rich in interest; in the whole of my wide field of research I do not remember one richer; but I must not dwell upon it beyond the relation of one or two facts.

I found that the vapour of the substance called amylene induced a sleep attended with a dream which might be called somnambulistic, during which acts the most natural were performed by the sleeper without consciousness of them or remembrance of them afterwards, the consciousness being, for the time, so obliterated that a painful surgical operation called forth no expression of pain or anxiety.

In investigating the action of another sleep-producing vapour, methylic ether, I discovered a still more curious dream; one, namely, in which the dreaming person would perform acts under the direction of the observer whilst quite unconscious of pain and without after-remembrance of that which had occurred during the sleep. This was a kind of artificially induced

hysteria, not unlike the condition known as hypnotism presented by sensitive and very susceptible individuals.

There is a medicinal plant which in the days of the Greek physicians, and from them up to the thirteenth century, was used as a narcotic. It was called Mandragora. Dioscorides gave a formula for making a wine, which got the name of *Morion* or death-wine, by steeping the root of the plant, Atropa Mandragora,—a plant which grows in the islands of the Grecian Archipelago, and is akin to our Atropa Belladonna, or deadly nightshade—in wine. Morion, or death-wine, when swallowed was capable of producing such a deep sleep that it veritably led to the phenomena of "shrunk death." This was the wine Juliet took. It caused a dream from which the sleeper awoke with screams and terror. They who by habit drank this substance uttered shrieks on waking, and from that probably the idea, ridiculous enough, gained belief that the plant itself shrieked when it was torn from the earth. I made this wine some years ago, and found that its properties were as stated. It produces its own special dream, a dream, even in lower animals that are susceptible to its influence, of anxiety, excitement, and alarm, ending in sudden return to consciousness in the midst of excitement.

A dream of a special and remarkable kind is induced by cannabina, the active principle of Indian hemp. My friend the late Professor Polli, of Milan, who experimented on himself with cannabina, found that the dream was recurrent; and the peculiarity of it was that within a brief period, even of a few seconds, events occurred to the mind that seemed actually to occupy an eternity of time. The facts gave direct

experimental proof of the theory that in dreams time plays no important part in the phantasy, but that a whole lifetime of story may be compressed into a minute of existence.

CAUSES OF DREAMS.

From the study of the phenomena of dreams we may pass now to that of causes of the phenomena, pass to ask why we dream and wherefore?

In considering this question with the scientific spirit we are led to see that the dream is a pure physical phase of life; that it depends on two conditions: our own corporeal organisation and the state for a time of the surroundings of our life. There is in it no more mystery, no more prescience, no more power, than there is in the dream of the wakeful day. We dream according to our nature, our habit, and our environment. Hannibal dreamt of Juno: he could not have dreamt of the Virgin, because he did not know of her; a good Catholic, in these days, would never dream of Juno, because she is to him a nonentity, but he might and, I am told, does dream reverently of her he believes to be the blessed Mother and Queen of Heaven, because to him she is both Queen and Mother. And so with all else in the way of dream; it is a partial mental activity combined with more or less complete physical repose.

The seat of dreaming is in the locked-up closet of mental impressions, the brain and spinal column, commonly called the cerebro-spinal centre, the absorbing centre of vibrations from the surrounding universe, the retainer of those vibrations, and the sender forth of them in the form of energy, witnessed in thought, deed, and word.

In the course of its vital activity this nervous centre wears out in its motor work, in its power of keeping the great muscles in play, more decisively than in its mental labour. So the muscular eyelids droop, and the limbs fail, and all except the vital involuntary movements of the heart and breathing muscles sink into rest for vital repair of their own structure, whilst the central battery, to use a simile as distinct from an identity, recruits itself. In this "twinkling of oblivion," the sentinels, the senses of sight, hearing, smell, taste, touch, common sensibility, which through their nervous cords pulsate the impressions they receive to their centres, more or less cease their offices. The eyes close first, for if they did not, sleep would be well-nigh as impossible as it is in the fish, which never seems to sleep; the ear reposes less readily, for if it did not, we should be exposed to many dangers we are aroused from by noise; the sense of odour, having its origin in the open nasal cavity, never closes, a fact which accounts for odours having so powerful an influence in dreams; the sense of taste closes; that of touch, situated in the finger tips, is in abeyance, but the common sensibility from the nervous expanse of the skin and mucous membranes is ever imperfectly closed, a fact which accounts for pressure and movement causing dreams or actual awakenings to full life. Thus between the outer world and the great centres of thought and feeling there are vibrations even in sleep, and when these reach their central points there is wakefulness, activity there, and that imperfect argument which we call dream, in which some centres are more or less active and some more or less absolutely passive, as if for the time dead.

These explanations of communication betwixt the outer and inner world of man account for the objective dream; but there are other communications, more personal if I may so express myself, which deserve to be considered. We have two nervous systems: one our own, by which we will and do; the other nature's, which goes on with our vital work whether we will or no. When we lay open the nervous casket, we see before us two brains, the great and the small, cerebrum and cerebellum, with their spinal cord and nerves communicating with sensitive surfaces and with muscles, all under our own rule and governance. But there is the other system, belonging to nature, centred within the trunk of the body, not in the closed box of the skull and spinal column, but in the line of the great viscera, to which its nerves are distributed, and in which it communicates with the nerves of the cerebral system, which are our own. In this second system lies the governance of the heart, of the digestive organs, of the breathing organs, to a considerable extent, and of the great secreting glands. How extensive this second nervous distribution is can only be understood when it is fully laid out before us in dissection, or in faithful picture from dissection. In one set of organs alone, those concerned in digestion, such a view conveys, at a glance, the richness of the supply of these involuntary nerves. Strangely also, these organic nerves combine with a nerve that wanders down to them from the cerebrum itself. Vesalius, the first great anatomist, traced this wandering nerve, or *par vagum*, and depicted it, not knowing of the organic nerves with which it comes into communion. Dissected out, this true wanderer is found to run down to the larynx, the œsophagus, the heart, the stomach, conveying intelligence to the brain

of any local disturbance and rousing up the great centres to exert themselves, and by a reflex notice to the muscles, forcing the muscles to do their best to remove any intruding cause of excitement and evil.

We have not yet finished. If we follow those filaments of organic nervous centres which are not our own and have nothing to do with our royal wills, we find them accompanying the arteries which carry the vital blood to the extremest destination of the blood-serving or arterial system, governing those vessels up to the finest twig, and, as the late Sir Thomas Watson simply but finely defined it, regulating the supply of blood to every part, as a gas tap regulates flame, so that the exquisite system of vessels, all and ever in senseless pulsation during our lives, is quickened, or slowed, or checked, by insensible direction, into various stages, into the redness of rage or the pallor of death.

In these mechanisms we see the origin of the subjective dream. That indigestion, perturbation in the richly nerved digestive organs, should lead to the transmission of vibrating and startling messages to the mental centres, is simple truth enough; that fever should excite, and that every influence or disturbance—friction, distension, heat—should disturb, in parts, the sensorium and conjure up a phantasy, is no longer a mystery. It is a phenomenon that must be.

Touching the effect of external influences I have one word more to add. Warmth of the air breathed by the sleeper favours dreams, while coldness of the air disfavours them. Hence the vivid, brilliant dream of the Asiatic, the dull dream of the Northern blood. I have been assured by one eminent Arctic explorer, the late Sir Edward Belcher, that the Esquimaux do not know what dreaming means, and our distinguished colleague

Dr. Rae, in a letter I have before me, says he does not recollect hearing of the phenomenon, although he thinks such an imaginative people as the Esquimaux may dream.

MOTOR AND SENSORY DREAMS.

The reference to the effects of cold brings me to an experimental demonstration bearing on motor and on sensory dreams. We all know from our common experiences that there are in us two powers: one of mind, the other of motion. If I were to enter fully into this experience I might be able to prove that the two powers are essentially one. It is enough now to explain, from what has preceded, that in the dream the motor powers sleep, as a rule, uninterruptedly, whilst the mental may be dreaming actively. We have, however, seen exceptions to this rule; and I may add that we can bring out such exceptions by experiment.

In some of my early experiments on the effects of extreme cold on nervous function, I found that the centres of nervous action could be reduced to such inertia by cold that deepest sleep was inducible, sleep leading to unconsciousness and perfect repose of all parts save those which are under the influence of the organic nervous ganglia and their fibres. Soon afterwards Dr. Weir Mitchell, of Philadelphia, and I, simultaneously and independently discovered that by putting different centres to sleep by cold of different intensities we could produce variations of motion, by influencing motor parts of the brain that balance each other. For example, we found that in birds the cerebellum, which in full activity impels the body to forward movements, is balanced by two great ganglia in the fore part of the cerebrum, called by the old

anatomists the corpora striata, and that if the cerebellum be put to sleep the body makes backward somersaults; while if the ganglia in the fore part of the cerebrum be made to sleep, the body is impelled forward in a similar mode of motion. There was thus produced one of the same conditions which Mr. Weston experienced in his dream after walking many short laps. He had wearied his cerebral centres by the constant jerk he encountered in his long exercise on the short circuit; and when he passed into sleep, his propelling centre, the cerebellum, which in its action had become almost automatic, seemed to force him forward imperatively. In this way dreams often become automatic where balance is not correct. In some persons a kind of sudden dream occurs when they look down a steep height; the controlling centre in the brain is for the moment overpowered, while, the propelling centre continuing unaffected, the danger is occasionally realised, of precipitation into the space below.

This is the dream of the motor centres in a state of broken balance, but there is a dream also of the reasoning centres due to broken balance in them, in which one nature in man seems to struggle with another, as if indeed two were contending, a dream of weariness and strife, such as is commonly present in the unhappy during waking dreams, as well as dreams of sleep. I have called this the dream of contention. The break of balance in this instance is between the two hemispheres of the brain, which, like the two hands, the two eyes, the two lungs, are independent organs, and which, as Dr. Wigan taught nearly fifty years agone, are by their independency the cause of the dual nature of the mind. If these hemispheres are closely akin in function, either for strength or weakness, we

have the evidence of the single mind for strength or for weakness. But in very few is there such equality: in the majority of persons one hemisphere is strong, the other less strong; the stronger ruling until it is so wearied that it gives way to the feebler. The apparent contradictions of human nature are readable by this key in workaday action; and so is the dream of contention, dream of resolution, of contrition, of remorse, in some cases of confession of real or imaginary offences against common or moral law. The sleeping dreams of the insane, like their waking dreams, are specially of this character, and afford the best insight we have into the meaning of what is called the unbalanced or insane character.

PRACTICAL LESSONS.

My reading of dreams, their phenomena, and their causes would be incomplete were I not able to draw from the study some useful practical lessons. We can draw many such lessons, and here are a few.

Dreams are all explainable on physical grounds; there is no mystery about them save that which springs from blindness to natural facts and laws. We make our dreams as we do our lives. They are reflexes of that which we take into our organisation.

Absence of dream in sleep is a sign, all other things being natural, of sound health physically, mentally, and morally.

Dreams occurring in childhood are invariably signs of disturbed health, and should be regarded with anxiety. If they are purely subjective they indicate some derangement of body; if they are objective they tell that some injurious food is being supplied to the developing mind.

A night of dream relating to events of the day is

a sure sign of mental overstrain; and the dream of continuation of mental work is a sign of danger which should never be disregarded. It becomes very quickly automatic in its course and injurious in its effect.

Dreams are a cause of mental weariness extending into waking hours, and when that fact is experienced the grand remedy is exercise of body. Exercise calls into play the centres of motion which have rested; and whilst they, with new associations, are in play, the mental centres rest and recuperate, which means in the truest sense *re*-creation.

It is an open question whether a dream ever leads to permanent disturbance of mental equilibrium, that is to say insanity. Dr. Hack Tuke has supplied me with a history which gives colour to an affirmative view of this question. I am uncertain on the point; but I am certain that dreams tend to mental aberration, and that every circumstance leading to them should be removed from persons of unbalanced mind.

To avoid wearying and wearing dreams, all objective influences which excite the mental centres should be under control. Sleep, in short, should be in the most noiseless atmosphere, as well as pure, where thieves break not in and steal the repose. How shall a man sleep dreamlessly who, by excitement of any kind for finding sleep, makes those arteries on which his brain is built beat two beats to one, hammering away at his senses and putting on them ten, fifteen, twenty foot-tons of pressure from the heart, as if it were a good experiment to find how quickly the delicate brain structure may be beaten into a solidity which natural vibration shall fail to call into natural function? But I must not tarry longer.

In this temple of science it is our business to

converse with the universe, using experiment as our interpreter. If we cannot explain we confess we are ignorant, and shall remain ignorant until time and circumstance bring insight. If we acquire knowledge we must speak of it with so much of understanding as is given us to plane and square and fit it into a shape that is understandable. Beyond this we cannot pass. Pardon me therefore if I have ventured to-night to speak of the "stuff" that dreams are made of on physical principles, and none other. It is in the spirit of our craft, and must be obeyed implicitly *sans* mystery.

POST SCRIPTA.

SUBJECTIVE DREAMS FROM CEREBRAL RESONANT PULSATION.

Dr. Cane, of Plumstead, has been good enough to remind me of an omission of a form of dream to which I had already drawn attention in ASCLEPIAD, Vol. VIII., p. 332—a dream produced by resonant cerebral pulsation. It is the dream of those who are suffering from constant noises in the head of a pulsating, or in bad cases continuous, character. This dream is special, and is most characteristic of a subjective origin. As a rule, the patient who may hear the pulsating murmur during the whole of the waking hours gains a respite as sleep comes on, and on falling asleep ceases to be conscious of what may be a persistent enemy to comfort so long as the mind is awake. But it is not always that such good fortune occurs. In some persons sleep is only obtained from sheer weariness and in spite of the noise, just as it may be in the clamour occurring from external noise, and then there is dream often of the most painful character. The dream is usually of tumult and

accident, attended with loud and discordant sounds, and sometimes with voices which shout and storm, but seem to say nothing understandable. One person informed me that in his case the noise was that of machinery in full work, and was wont to recall to him "life on board ship in a bed near the engine." He was led frequently to wake up with the idea that he was on board a vessel in which he had more than once made a long voyage. The noise in this instance reawakened memories, and led to two imperfect trains of thought more or less combined, and to a partial realistic phantasy, subjective and objective. Touching this same part of the subject, Dr. Cane suggests that there are "internal sources of light in the phosphorescence of the leithin, cerebrin, and other parts of the nervous tissues," an idea suggested to him by noticing how like a dream the great dark surface of the ocean is when lit up by shoals of phosphorescent animals. This idea is sustained by the experiment, which most persons can realise at the moment of going to sleep, of closing the eyelids and looking, if I may put it so, at the inner surface of the eyelids. The space looks large and luminous, and at the bottom of it there are seen evanescent, half-defined forms, usually faces which bear some sort of resemblance to known faces, but are too dissolving, and oftentimes too grotesque, to be identified particularly, although they may bear the general impression of faces that have been seen. These are, no doubt, dreams from light, projections from the written centres into space of reflections imperfectly focussed, and appearing rather as shadows than substantials. On a larger scale the projection called commonly a ghost may appear in this manner—a subjective dream modified by objective phenomena.

DREAMS OF THE ESQUIMAUX.

From an incorrect report of the lecture I was made to state as a fact that the Esquimaux never dream; but, as the reporter put it, "sleep like icebergs." No such statement came from me. The question opened is, however, important as well as curious. Dr. Rae, in a first note to me on the point, was not sure; and in a second communication he "thinks Esquimaux do dream, but he cannot remember anything to prove it."

At the same time he supplies another bit of valuable information as follows:—

"Dreaming is common enough in the cold north among visitors who go there, especially under certain circumstances. On one of my Arctic sledge expeditions we found the ice so rough and difficult to get over, we had to leave our sledges behind and carry everything on our boats, which made it very hard work and our advance so slow that we ran short of provisions. In this time of privation, which lasted about a week, we dreamt every night extensively, the subject of our dreams being almost invariably of some special dish or kind of food we were, or had been partial to in our youth. As there was an Orkneyman, a Highlander, a half-bred, and a Canadian of the party, there was a considerable variety of diet mentioned as we recounted our dreams in the morning. When we had got enough of food we were not troubled with dreams, and our day's work of ten to twelve hours acted as an excellent soporific."

OPUSCULA PRACTICA

"There are mites in science as well as in charity."
BENJAMIN RUSH.

WORK IN THE LONDON TEMPERANCE HOSPITAL.

AFTER five-and-twenty years of retirement from hospital duties, I have recently returned to them by the invitation of the Board of Management of the London Temperance Hospital, a vacancy having occurred there on the resignation of Dr. Edmunds, and I propose to record, from time to time, in these columns such details of practice there as may seem of interest to ASCLEPIAD readers. As preliminary to such work it will be well to supply a few facts relating to the principles that are carried out in the hospital. The hospital has been established in order to place on the broadest basis the truth that diseases of the acutest, severest, and most varied character can be treated successfully without the employment of alcoholic beverages. At the same time no attempt is made by the management to prevent the members of the medical or surgical staff prescribing alcohol if in their judgment it be demanded as a means of cure. The only condition that has to be complied with is the first condition: that if a physician or surgeon of the institution prescribes alcohol, he shall initial the prescription so as to show that he is responsible for it.

In all other particulars the rules of the hospital and the management are the same as in any other general

hospital. The staff consists of one physician, myself, having no out-patients; two physicians, Drs. Ridge and Fletcher Little, each of whom has five beds under his care and has charge of out-patients; a surgeon, Dr. Collins, who has a clinique of in-patients, amounting to thirty-five or more, and who has also charge of the surgical out-patient department; two resident house surgeons, Drs. Stubbs and Haydon; and a registrar, Dr. Wilde, who is not resident. There is also an experienced matron, Miss Orme, and an excellent staff of twenty-two nurses.

All cases are admitted except they be infectious cases. For reception of the infectious there is a separate ward for use in emergency, but it is not the rule to retain for treatment or to admit for treatment diseases of an infectious character.

The medical and surgical wards are distinct, but there is a children's ward, in which medical and surgical cases are intermixed.

The sanitary arrangements of the institution are well planned; the wards are freely ventilated, equably warmed at a mean of 60° Fahr., nicely decorated, and kept in excellent neatness and order. There are balconies to the wards covered in above in which those patients who are able to do so safely can, at proper seasons, walk or sit in the open air. There is also an extensive public ground at the back of the hospital, in which the convalescent can take daily exercise.

The dietary is carefully supervised. There are three kinds of diet: low or milk diet, middle diet, and full diet; there is also what is called a fancy diet, in which special foods are supplied according to the wishes of the physician or surgeon, but neither wines, beers, nor spirits form any part of the dietetic systems.

The hospital is situated in a poor neighbourhood of London, or perhaps it would be more correct to say in a neighbourhood in which there are many poor. Patients are admitted whether they be or be not abstainers from intoxicating drinks, and large numbers of non-abstainers are admitted and are as successfully treated as the rest. This is an important point for remembrance in regard to the strict rule of abstinence that is carried out, and it is a remarkable fact that the patients of the non-abstaining class when once they have entered the wards and come under the rules, rarely if ever, except in instances of "drink crave," ask for any of their accustomed spirituous beverages. It is as if the entire absence of the tempting drink led to a quick moral influence, under which habit is overcome. I have never had a single request made to me for vinous drink nor a single complaint at its not having been supplied.

In the purely medicinal department alcohol ranks as a drug, simply or in combination with other medicines. For tinctures glycerine is used as the menstruum or solvent; and it is only when alcohol is absolutely required in the preparation of tinctures that it is employed.

OATMEAL PYROSIS.

AN obstinate case of pyrosis came under my care some years ago, and resisted all forms of medical treatment. The dietary of this patient, an orderly and temperate man, seemed to call for little supervision; in fact, I never remember to have had under my care any one who was simpler or more methodical in the way of feeding and

drinking. I recommended him to leave off tea, and he obeyed, but with no beneficial result. Coffee was withdrawn, with no better result. The patient was an abstainer from alcohol and did not smoke, so reforms were not called for in those directions. For a time he left me, and consulted another physician, I believe more than one, but after a few months he returned, complaining of the continuance of the symptom that so oppressed him. He really suffered acutely. Each morning, between breakfast-time and luncheon-time, the water brash came on. The quantity of fluid ejected from his stomach was considerable, and the ejection was preceded by a sense of sinking at the heart and faintness which sometimes alarmed him. By the free discharge of the fluid from the stomach he was invariably relieved, and rarely had any return of symptoms until the following day.

The story of the repetition of the attack, always at the same or about the same hour, was so peculiar and so often repeated, I could not fail, at last, to attribute it to something taken at breakfast; and, finally, I began to suspect that a dish of oatmeal porridge might be the enemy. He had taken this for breakfast for many years, and had never thought it injurious, and when I named my suspicion he was incredulous. However, he took the advice to leave off oatmeal "on trial," and from the day of leaving it off had no return of his symptoms. Six months later he ventured the oatmeal diet again, and in a week was as bad as ever. Once more he left it off, and once more was completely cured. This was observation on a patient; but for experiment's sake I tried the effect of oatmeal diet on myself, with the result of setting up in a few weeks as decided an attack of pyrosis as could be observed or felt. In my

own case I found that barley-water, repeated for a time, produced the same result.

After making these observations I continued to inquire, in all instances of pyrosis I have since met with, whether oatmeal formed a part of the dietary of those affected; and I have found so many corroborative experiences, I am led to think there is no more frequent cause of pyrosis, than oatmeal or a similar fermentative food. Hence the term at the head of this opusculum, "Oatmeal Pyrosis."

PURULENT DEPOSITS WITHOUT RIGOR OR PYREXIA.

AS a rule, to which there are few exceptions, the presence of pus in the cavities of the body or in the cellular tissue is marked by intermittent attacks of rigor and fever. On this point it is not necessary to dwell, the fact is so commonplace. But with the fact straight before us, there arises a question difficult to solve, namely, whether rigor is due to the actual presence of pus or whether such presence is merely coincident and is indicative of some other influence affecting the nervous system, distinct from the pus as a cause. I am led to think that the mere presence of pus locked up in a close cavity is not alone sufficient, and for these reasons. (1) Rigor is often present in a severe form and at regular periodical times when it is certain that pus does not exist anywhere. (2) In what is commonly known as "cold abscess" there may be all the physical signs of pus localised in an accessible part of the body, and yet there may be no rigor. (3) Pus may be present in cavities of the body in large quantities, and yet there may be

no rigor. Quite recently I had in the hospital a patient suffering from emaciation, enlarged glands in the groin and axilla, constant cough, and expectoration of mucus derived from the bronchial surface, with other signs suggestive of effusion into the pleural cavity, but without any touch of rigor or any such pyrexial exacerbation as would lead to the diagnosis of empyema. Yet after the death of this patient, which occurred from asthenia coupled with hydrops bronchialis, there were found in the pleural cavity several ounces of pus, with purulent deposits in other parts. In brief, the presence of pus was not alone sufficient in this instance to call forth the nervous disturbance which, under the name of hectic, periodical rigor, or remittent pyrexia, we usually connect with purulent exudation. Subject to correction, I am at this moment inclined to think that pus is a determining cause of rigor only when it exists in conditions favourable for its absorption into the circulation, and when it keeps the space it occupies full by reason of the fact that it is always being made and is always being removed by absorption. In "cold abscess" pus is so circumscribed that it cannot be absorbed and cannot accumulate to tension. It lies embedded as if it were a simple or benign tumour; and except for some pain it may cause by pressure or some inconvenience it may induce by enlargement, it is negative in its effects. In the case I have related, where extreme asthenic conditions prevailed, there was a different condition, but leading to the same end. The circulation was so feeble, and the power of vital action generally so impeded, there was no absorption even from a serous cavity. The pus, in these instances, lies *in statu*, and is, therefore, not toxic; that is to say, it does not by circulating in the blood set up excessive

zymosis, and does not so increase in volume as to produce tension or lead to local irritation of nervous fibre in peripheral distribution: there is, therefore, no extra vibration communicated from the surface to the centres of the nervous system.

The view here suggested explains correctly, I conceive, the reason why the letting out of pus confined in a cavity or abscess succeeds so well during acute affections in preventing recurrence of rigor, and why the continued escape of pus, within limitation of exhaustion, is so advantageous. By the free escape we prevent both absorption and tension, a double security.

OXYGEN AS A MENSTRUUM FOR INHALATION.

IN 1887 I drew attention in these pages to a novel mode I had then introduced of using oxygen in the treatment of disease. The mode consisted in utilising oxygen as a carrier or bearer of other medicaments that can be administered by inhalation. I began, it will be remembered, by making oxygen the bearer of ether, and I observe that *ethereal oxygen*, as I called it, has gained admission into the extra pharmacopœia. I have since extended the use of oxygen as a bearer or carrier to such a further degree, it may now be considered a determinate addition to therapeutical administration.

Besides sulphuric ether, oxygen can be employed as a menstruum for ethylene, chloroform, methylene, nitrite of amyl, methylal, nitric ether, ammonia, ammonia with alcohol, iodine, bromine, benzoin, turpentine, and the volatile oils; and, as it is the prime supporter of animal combustion, the inhalation of it in combination

with the substances it bears may be continued for half an hour at a time without a moment's interruption. This, in a practical point of view, is of immense importance, because it enables us to administer a considerable and effective dose of an active medicinal substance so imperceptibly that the patient is scarcely conscious of inhaling anything more than an ordinary and pleasant air.

In so far as I have proceeded, hitherto, in this direction, I have administered by the following methods. I have obtained the oxygen from the decomposition of hydrogen peroxide by permanganate, as already described in previous numbers, making the oxygen the medicine-bearer by letting it pass through volatile substances on or in the peroxide solution. This plan answers well for all volatile bodies that will float on the solution, like ether or ether holding iodine, and I have used it with excellent results. The objection, and the only objection, to the plan lies in the cost of production of oxygen from the peroxide solution, and this has led me to use compressed oxygen, allowing it to pass in gentle current through the medicated solution into the inhaler. For hospital purposes I have had a table fitted up that can easily be wheeled from bed to bed with the store of compressed oxygen underneath and the bottle for medicated solutions fixed on the top.

In cases where water is not admissible I follow out another system. I place the volatile substance, say iodine or turpentine, in a good-sized flask with a double neck, and let the oxygen simply flow over it on its way to the inhaler.

Again, I sometimes charge an elastic receiver with oxygen that has passed over the volatile substance to eb inhaled. The patient so provided with a definite

charge of medicinal substance is able to have it by the bedside, and can inhale according to the direction given. The bag which the late Mr. Clover made for the administration of chloroform, as a modification of Snow's balloon, answers best of all for this purpose. The bag, as manufactured by Messrs. Coxeter, is lined on its inner surface with a varnish which is flexible, and which is not acted upon by the vapours or gases introduced into it; it is easily charged with the medicated gas, and it can be charged in measured quantities, so as to supply an exact dose.

The advantages springing from the employment of oxygen as a menstruum are so obvious, it is certain that in course of time the method will be of wide application. The effects produced are rapid and decisive: the distribution of the medicine through the blood is, like the oxygen itself, to all parts of the body; the digestive system is not directly interfered with; and the precision of administration is one of the most desirable attainments springing from the method. In a future number I shall be able to supply some illustrations of apparatus, with, probably, further improvements and simplifications.

A CELLULITE MOUTHPIECE FOR INHALATION.

TO the apparatus for the inhalation of oxygen described in a previous volume (1884) I have now added a new and convenient mouthpiece, constructed for me by Messrs. Robbins, of Oxford Street, in which exceedingly light cellulite, artificial tortoise-shell, forms the basis. The mouthpiece is double-valved, and adapts itself very neatly to

the mouth of the patient. The great advantage, however, consists in the fact that, like glass, the cellulite substance can be washed and rapidly cleansed and dried, so that one patient can use it immediately after another with perfect safety. This is an important point gained, in a preventive sense. Leathern inhalers soon become impregnated and unclean when they are in frequent use; rubber inhalers are liable to the same objection, and vulcanite ones are not entirely free from it; add to which they are all clumsy, heavy for the patient to hold, and not easily adaptable. The cellulite mouthpiece meets these objections; it is as good for administration of chloroform, methylene, or other volatile body, as for the oxygen apparatus, from which it can be easily detached for other purposes.

ANODYNE SINAPISMS.

MUSTARD can be used advantageously as the menstruum for anodynes in the form of external application, and without destroying its value as a counter-irritant. The mode of application consists in mixing the mustard into a thick solution either with olive oil or with glycerine, and then incorporating the anodyne, opium, tincture of aconite, cocaine, belladonna, or other narcotic. The vascularity caused by the mustard favours absorption, and an insensibility amounting to a slight local anæsthesia can be induced by this simple method.

SIR KENELME DIGBY, F.R.S.

"WHEREAS, upon the mediation of her Majesty the Queen of France, it hath pleased both Houses of Parliament to permit me to go into that kingdom, in humble acknowledgment of their favour therein, and to observe and confirm a good opinion of my zeal and honest intentions to the honour and welfare of my country, I do here, upon the faith of a Christian and the word of a gentleman, protest and promise that I will neither directly nor indirectly negotiate, promote, consent unto, or conceal any practice or design prejudicial to the honour or safety of the Parliament. And, in witness of my reality herein, I have hereunto subscribed my name this third day of August, 1643.

"KENELME DIGBY."

The "deed of honour" given in the above paragraph is characteristic in full of its author, of the time in which he lived, and of his social position and character at the moment when the document was sealed, signed, and delivered. He had been for some months confined in prison at Winchester House, by order of the Houses of Parliament, for having, at the instigation of the queen

of Charles I., taken part with Sir Walter Montague and other Royalists in persuading the Roman Catholic community to raise a subscription army for the King in support of his defence against the troubles that were close upon him. The scheme, to a considerable extent, succeeded in so far as the contribution of money was concerned; but it was very far from being a success politically and socially. The kingdom at the moment was Puritan in heart, and was, of all things, resolute that neither Pope nor Cardinal should tithe or toll in its dominions; and the army that was raised by the money of the papal adherents who were friends of the King got the name of the "Popish Army," as bad a title as could be applied to it in the tone, then, of the national mind. The Parliament, consequently, had seized Sir Kenelme as one of the promoters of the new device of a force that might be soon arrayed against itself and the liberties of the people, and in 1640 it subjected him to question as to the reason of his conduct and the extent of his action. From what ultimately transpired we may gather that the members of the Parliamentary committee who were concerned in the inquiry were struck by the candour of the man who was brought before them. He despised every kind of subterfuge or evasion. He told them clearly what it was he had done and why he had done it; and for a time he was left free. But when, somewhat later on, the civil war commenced in earnest, the courageous and in many ways powerful knight was laid hands upon and committed, as we have seen, to confinement. From this he was liberated, at the date named in his letter, on a request to the Parliament Houses from the Queen Dowager of France, communicated to them by the Sieur de Gressy, on the condition that the prisoner

signed a declaration, such as he did sign, pledging his good faith that he would do nothing prejudicial to the honour and safety of the Parliament.

Readers who are not conversant with the life and character of a man in the position of an opponent to Parliamentary government in 1643, a knight raising forces for a king, and a friend, obviously, of a ruler in another country, will wonder how such a man can be brought into a history of medical scholars. Was he, Kenelme Digby, a physician or a surgeon? In the strictest sense he was neither, for he took no university nor other title bearing, classically, on medical or surgical art. He was really a fighting man by land and by sea; he was a politician on the side of royalty; and he was independent of any professional ties, except in arms and politics. And yet, like many others, he is bound to our brotherhood by more than one act bearing on medical and surgical art. To the learning and diplomatic skill which were natural to him, and in which he excelled most markedly in the eyes of his cotemporaries, he added a love for natural philosophy, and made, thereupon, some additions to philosophical medicine which hold their influence even to the present day. It has been said of him, rather despitefully, that he was "a leech," by which term knights who took part in the treatment and cure of the wounded in battle were often known. In old days, when the licence to kill or cure was not demanded by law, these leeches were free to practise, and were very free in suggesting lines of practice of the boldest type, in which labour they often rendered excellent service. Sir Kenelme Digby was an illustrious representative of this order of generally useful men, pioneers of Science in her practical applications.

From Childhood to Manhood.

The childhood of the man now before us is one of sad and painful romance. When he was three years of age, his father, Sir Everard Digby, suffered death on the gallows, and was, to use the common phrase, "hanged, drawn, and quartered," at the west end of Old St. Paul's in London. This tragical act was performed for his having taken part in the treason of Gunpowder Plot, a scheme for wholesale destruction of King, Lords, and Commons, that resembled closely the dynamite plots of the present day. Sir Everard had no actual part in the details of this miserable scheme, but, on his own admissions, during and after his trial, he entered into the idea of it at first, received the notorious Guy Fawkes at his house, and, for a time after his own seizure, persistently refused to give up the names of the other conspirators. At his trial he pleaded guilty, assigning for the reason of his guilt the argument that it was the duty of a true Catholic to attempt, at all risks and hazards, to restore the true faith to the English nation. When the judges had condemned him, he bowed to them, observing, "If I could hear any one of your lordships say you forgave me, I should go the more cheerfully to the gallows," to which they all replied, "God forgive you, and we do." Our old friend Wood, to whom all writers on biographical events occurring in or about his own time are so much indebted, reports respecting the death of Sir Everard that when the executioner plucked out the heart of his victim and, according to custom, cried to the people, "Here is the heart of a traitor," Sir Everard replied, "Thou liest."

The fate of the father was not reflected in any way on the son whose life is now before us. Little Kenelme

was the eldest child of Sir Everard, and had one brother, John, who, like himself, afterwards rose to great fame. The boys at the time of their father's death were with their mother, Mary Digby, *née* Mary Mulsho, at Gothurst, in Buckinghamshire, where the family lived on an estate which had descended to Lady Digby from her father, William Mulsho. The birthday of Kenelme was June 11th, 1603. His mother was not for long allowed to retain him by her side, for she too was suspected to be of "Popish belief," and the child must needs be taken from her, made to accept the new or Protestant creed, and placed under the tuition and care of Laud, then Dean of Gloucester, afterwards doomed to die on the scaffold as the renowned Archbishop and assumed traitor to the Commonwealth. The family estate remained to the boy, who, under the care of Laud, became rapidly a scholar of first promise. He grew up in health and of robust form; and in his fifteenth year was entered as a gentleman commoner at Gloucester Hall, Oxford. He remained in college, the admiration of his tutor and fellow-students, nearly three years, and then setting forth on his travels abroad, made the famous round of France, Spain, and Italy, became "italianated" as the saying went, and returned home accomplished in all the arts and sciences of his time. It was in this journey he learned what has been considered the grand secret that connects his name with medicine; and, soon after his return, whilst staying at Lord Montague's house at Hinchinbrooke, he was introduced to the King, James I., who, forgetting his father's delinquencies or, more correctly speaking, overlooking them, and admiring the abilities of the son, conferred on Kenelme the order of knighthood in the year 1623.

King James died in 1625, and Charles I., on his accession, took the young knight into favour so warmly that Sir Kenelme Digby soon became quite a favourite at the Court. He was appointed a gentleman of the bedchamber, a commissioner of the navy, and one of the governors of Trinity. His work as a naval commissioner, moreover, was not merely honorary; it became practical. He was made a general or admiral of a small fleet which was sent out in 1628 to chastise the Venetians in the Mediterranean. He reached Algiers in the summer of that year, rescued a large number of English slaves, and finding the Venetian fleet in the Bay of Scanderoon, attacked it there with success, and returned home a renowned man.

Scholar and Philosopher.

Up to this period of his career Sir Kenelme Digby was generally recognised by his connection with public affairs, apart from any kind of philosophical learning or aptitude, but now he entered the ranks of the men of science and philosophy. His admiring tutor at Oxford, Thomas Allen, of Gloucester Hall, who, struck by the breadth of his knowledge, compared him to the famous Pico Mirandola, died in 1632 and left him a collection of literary works, in manuscript and in print, which were considered to be of great value. The bequest was a treasure so esteemed by the students of the university that, regret having been expressed at the loss of the collection by the university, Sir Kenelme in 1633 quieted the alarm by generously giving every work to the Bodleian Library. There were two hundred and seventy-seven manuscripts, of which thirty-six were in Hebrew and Arabic. They were sent by him to the Dean of Gloucester, Laud, to be forwarded by the Dean

with some of his own. In this way they passed in the Bodleian under Laud's name until a few years ago, when the present learned and able librarian, Mr. E. W. B. Nicholson, discovered the actual facts and refereed the works as "MSS. Digby Oriental," for which he deserves the best thanks of all scholars interested in the biography of the remarkable donor of so rich a gift to the great university library.

Not long after these events in the course of his life, a change came over the mind of this hitherto fortunate man. He had been brought up as a Protestant, and had adhered to the new system both as student and commander. Probably he had not hitherto paid close attention to the question of religious faith or belief, and almost certainly he had, so far, lent himself to the influence of Laud, whose powers of persuasion were of the most effective nature. Now, however, Sir Kenelme began to have doubts in favour of the Church of his fathers, doubts not surprising when the hereditary tendencies of his family are remembered. He returned to Paris probably in the year 1634, and whilst there he reviewed the history of the separation of the Protestant community from the Church of Rome. The result of his inquiry was to lead him back to the faith in which he was begotten, and which in the earliest days of his life had doubtless been instilled into his mind. The fact of this change in his life he first communicated to Laud, who reasoned with him without, say the biographers, having much hope that his arguments would take effect. Subsequently, in 1636, he published a treatise entitled *A Conference with a Lady about the Choice of a Religion*, in which, like some later converts to the ancient Church, he vindicated his action with moderation and brevity. He afterwards entered into a

correspondence on the same subject with his cousin, Lord George Digby, showing in his letters, which have been retained and published, the same moderation as characterised his previous treatise.

A letter referring to this controversy, which bears date from London, shows that Sir Kenelme had returned from France to his native country in December, 1638. His return was, we may fairly assume, for the political purpose noticed in the opening page of this article. He had in view the attempt to raise a papal army for the King, at the instigation of the most confirmed woman-plotter that ever lived—the Queen Henrietta. The attempt, as we have seen, failed, and the chief partner in it, seized when the real troubles were beginning, was placed as prisoner in Winchester House, whence he was released on signing the declaration that he would take no part in infringing the liberties of the Parliament.

It was natural, after signing so important a document—a document which did in fact admit a certain measure of complicity in a design for royal, as distinguished from popular, power—that Sir Kenelme Digby should once more seek the protection of France. He left for the French capital in the latter part of 1643 or early in 1644; but it is worthy of note that before he left he was brought before a committee of the House of Commons to give evidence bearing on the conduct of his old friend and master Archbishop Laud. Laud had been suspected of an intrigue with the Court of Rome. It was largely believed that his wishes tended towards Rome. The common belief or suspicion, a belief or suspicion that is not, in relation to his history, extinguished altogether even at the present hour, was that the Archbishop was negotiating for a cardinal's

hat, on the condition of his conversion and the restoration of the Romish Church in these islands. Digby, it was assumed, must be conversant with any design of the kind if it existed, and for this reason the Commons, who had recently released him from prison, called him up for examination on the question. The answer obtained was all in favour of the prelate; it was to the effect that the witness knew nothing whatever of the suspected intrigue, and that he believed the Archbishop to be a sound Protestant. The fact that the answers given were accepted in good faith, although they came from one who had recanted, and who had actually been a prisoner of the Parliament, affords perfect evidence of the confidence felt in the honour and integrity of the man who gave them.

The residence of Sir Kenelme Digby in France, recommencing in the year 1644, extended until the triumph of the English Parliament over the King was complete. During this exile he became the friend of the illustrious René Des Cartes, with whom he held frequent converse on various subjects in the realm of philosophy. For his first interview with Des Cartes Digby went to Holland, whither, at Egmond, the great philosopher had retired. The account of the meeting of these two remarkable men has been retained by Desmaizeaux, and is curious as affording a picture of compliment between two courtiers of philosophical type. It is told that Des Cartes did not at once reveal himself to his visitor, but conversed with him first on some subject of philosophy bearing on points upon which Digby had written. Then he added, "I do not doubt you are the famous Sir Kenelme Digby," to which the "famous" responded, "And if you, sir, were not the illustrious M. Des Cartes, I

should not have come here for the purpose of seeing you."

There was so much in common between Sir Kenelme Digby and M. René Des Cartes, it affords little cause for surprise they remained friends. They were both inclined to physical research, and they were also both inclined to metaphysical studies. This is but saying that they were imbued with a poetic as well as a scientific spirit, and under the dual influence tried to reach the unattainable through the attainable. The career of Des Cartes is singular in the matter here named: he began as a most zealous student of the most exact of the sciences, geometry, accounting for nothing except by absolute demonstrative argument. He ended, I believe, by an argument on the rule that two and two make four because "God willed it so." The argument would have suited Sir Kenelme Digby to the letter; he too began with the study of the exact in knowledge, but early in his career passed over to the mystical, and, without letting the mystical master the practical, explained or tried to explain the practical through the mystical, a hopeless task, and yet in some minds even in this day as strong and as inevitable as the dual nature of man himself. It is belief in contest with knowledge, with wisdom as a mere spectator.

SIR KENELME AS A PHILOSOPHER IN THINGS MEDICAL.

It is time now to refer to Sir Kenelme Digby as an author and to glance at the philosophy which appears in his works, and especially in that part of them which bears on the philosophy and practice of medicine. I have said that he was not directly connected with the practice of medical or of surgical art. He probably

never wrote a medical prescription as Harvey or Willis did; probably he never performed a surgical operation as Wiseman did; but for all that he was looked upon as learned in physic. The impression conveyed to me about him by one of my own early teachers was that he was a kind of learned charlatan, an observant scholar who dabbled in medicine and who, in an accidental manner, alighted upon one or two curious facts which the profession of medicine turned to account. We gather somewhat of the same tone from the writings of Dr. Paris. In the "*Pharmacologia*" Paris deals with Digby, not at great length, but with considerable mastery and with an offhand admiration. The feeling is natural. Men of physic, like men of all other professions, dislike, above all things, to acknowledge outsiders, whatever may be their skill, as within the pale, as men of other callings are equally determined not to acknowledge men of physic in their pale. Smollett was a man of letters, but to hold that position he had to give up the practice of medicine; and even then the "M.D." attached to his name injured his literary fame. Goldsmith had to be sacrificed as a medical doctor before he could be accepted as poet, playwright, story-writer, and historian. If Keats had recovered from the illness that brought his life to a close and had continued in our ranks, according to his own intentions, John Keats, surgeon and accoucheur, he would never have been "a joy for ever" as John Keats, poet and author of one of the most familiar lines in all poetry; his name would then, indeed, have been "writ in water." And so, turning the argument round, no man, whatever his qualifications, can hold his place in the history of physic unless he belongs to it actually and in a certain sense depends upon it as a profession

Sir Kenelme Digby has, therefore, never been considered, and is never considered by us, as in our camp in the strict sense of the word.

The only common ground for outside medical scholars and inside medical scholars lies in what is called philosophy. Philosophy covers everything in learning, and a man may be a philosopher on things medical and yet not be a medical philosopher. It is a fine distinction, but it has always, and always will, probably, exist. Celsus stands before us in this light, and many more that might be named. Here, too, I should place Sir Kenelme Digby; he was a philosopher in matters medical; his tastes ran with medicine; he studied some medical subjects, not with a trained or mechanical eye, as one to the manner bred, but with a natural and earnest inquisitiveness, which brought forth useful knowledge tinged with mysticism, ridiculous, perhaps, in theory, yet still useful in practice, and in the end explanatory in its scientific development. To do him proper justice, Sir Kenelme Digby was in matters medical a mystic as distinguished from a charlatan; there is not a shadow of evidence that he ever traded on his medical acquirements; he might have done so and have made a great fortune, but he did not, for which all honour to his reputation! Moreover, he was as loyal a scholar to physic as any scholar of his date could be. He read our best authors; he was loud in his admiration of William Harvey, whom he knew personally; it is certain that he was in possession of the best medical labours of his time, and he distinguished ably between the good and the bad. Let us accept him, with this knowledge of him, and taking him as he was, see something of what he taught bearing on our science and our art.

On the Nature of Bodies.

I notice, in the first place, a treatise by Sir Kenelme on *The Nature of Bodies*, in which he deals with "quantity, rarity, density, elements, light and what it is, local motion, gravity and levity, reflection, modulation, refraction, compositive qualities and generation of mixed bodies, rarefaction, condensation, attraction, filtration, restitution, electrical attraction, loadstone attraction; plants and animals, and how they are formed in common to perform vital motion; the generation of animals; figures of plants and animals; beginning of motion in living bodies, the motion of the heart, the circulation of the blood; nutrition, augmentation, and corruption or death; the motions of sense, touch, taste, smelling, hearing, sight, and colours; sensation, memory, voluntary motion; the passions; the material instruments of knowledge and passion; pain and pleasure; the reasoning of beasts, and the docility of some irrational animals."

Of Plants and Animals and Vital Motion.

A curious description of magnetic phenomena, in which the labours of William Gilbert,—who is classed with William Harvey as one of the two great lights in the philosophy of the age they adorned,—lead to a chapter on the two sorts of living creatures, plants, and animals, and how they are formed in common to perform vital motion. Plants in his view are one continuous substance, wherein we observe one and the same line of progress throughout, from the highest to the lowest part, so that the operation of one part is not at all different from that of another; but the whole body seems to be the course and thoroughfare of one

constant action, varying itself on divers occasions and occurrences according to the disposition of the subject. Animals differ from this in that their parts are notably separated the one from the other, and each of them has such a peculiar motion, proper unto it, that one might conceive they were every one of them a complete distinct total thing by itself, and that all of them were artificially tied together, were it not that the subordination of these parts to one another is so great, and the correspondence between them so strict, "as plainly convinces that the compound of all these parts must needs be one individual thing."

He illustrates his meaning in this matter by reporting on two machines he saw at work—one in Toledo, the other in Segovia—when he was travelling in Spain. The engines were moved by water, and one of them performed one kind of work alone; the other, "a multitude of engines," flattened into a plate an ingot of silver, delivered the plate it had made to another section that printed it as a coin and passed it over to a third that cut it to a proper weight and shape, and so on. Both engines derived their power from the same source, like plants and animals; but one was like a plant, the other like an animal.

Some exceedingly curious passages occur in this work of Sir Kenelme respecting the cause of the motion of the heart. Here he shows his admiration of Harvey, and pits him against his friend Des Cartes. Des Cartes had a curious notion. He fancied that the blood is forced out of the cavity of the heart all over the arterial circuits by pressure of steam generated from the blood within the ventricles. To this Digby replied that the heart has a power of motion in itself, that could be witnessed in such a reptile as the viper long after death

and when all blood is removed from it; and although he did not understand the heart as a muscle, he clearly saw that it acts after the manner of a muscle, and that it obeys the law of contracting when it is full of blood, by virtue of its own contractile power and independently of any force generated in it by the blood. At the same time he detected that the power of the heart is fed by blood, and he was, I believe, the first to assert that the organ is made up of different sets of fibres: one set "the straight ones, which go long ways on the sides of the ventricles from the thick basis of the heart towards the little tip or cone of it; the second round ways about the ventricles within the heart; and, the third transversely." These different arrangements of fibres give, he thought, an independent motion to the systole and diastole, and he ventured, correctly, to disagree with Harvey himself on the diastole, which is not, he thought, a mere "relenting from motion," according to Harveian doctrine, "but is a complete motion, and in a manner greater than the systolic contraction, though less sensible."

On Death and Sickness.

There are many more remarkable passages in this work which I should like to present. I must be content with one or two. In a chapter entitled "On Death and Sickness," I would have him properly heard. I give therefore a passage entire:—

"In the essential composition of living creatures there may, peradventure, be a physical possibility for them to continue always without decay, and so become immortal, even in their bodies, if all hurtful accidents coming from without might be prevented. For seeing that a man, besides the increase which he maketh of

himself, can also impart unto his children a virtue by which they are able to do the like and to give again unto theirs as much as they received from their father, it is clear that what maketh him die is no more the want of any radical power in him to increase or nourish himself, than in fire it is the want of power to burn which maketh it go out. But it must be some accidental want, which Galen attributeth chiefly to the dryness of our bones and sinews, etc., for dryness with density alloweth not easy admittance of moisture; and therefore it causeth the heat which is in the dry body either to evaporate, or to be extinguished; and want of heat is that from whence the failing of life proceedeth."

Teaching the Dumb to Speak.

A chapter on the senses and their mode of action is rich in suggestion and often advanced in knowledge. Digby knew perfectly well that the senses of taste and odour were due to the impressions of fine particles; he knew that the sense of hearing was due to waves of air, and he experimented on himself by lying under water in order to learn how far waves of air above the water would cause waves in the water, which, being communicated to his tympanum, would produce sound; he had an excellent appreciation of optical laws and of the sense of sight; but the most important and curious portion of his work here lies in the pages where he shows "how one sense may supply the want of another."

There is truly nothing new under the sun. We have prided ourselves that in our day we have learned the art of teaching the dumb to speak. We call this beautiful art "lip-reading"; Sir Kenelme Digby called it "hearing by the eyes." A Spanish nobleman, brother of the Constable of Castile, was born deaf, so deaf that if a

gun were shot off close to his ear, he could not hear it, and "consequently he was dumb, for, not being able to hear the sound of words, he could never imitate nor understand them." Many attempts were made in vain to remedy the defect. At last a priest undertook the teaching of him to understand others when they spoke, and to speak himself that others might understand him; "and what at first he (the priest) was laughed at for made him after some years be looked upon as if he had wrought a miracle. In a word, after strong patience he brought the young lord to speak as distinctly as any man whosoever, and to understand so perfectly what others said that he would not lose a word in a whole day's conversation."

The priest, who wrote a book on this subject, was alive when Digby was in Spain with the Prince of Wales (afterwards Charles I.), and both the Prince and he saw the brother of the Constable who had been taught to speak. The Prince, who had, as we know from the life and work of William Harvey, a great liking for natural science, was as intensely interested in this so-called miracle as Digby himself. They observed about the voice of the deaf-mute that, "not hearing the sound he made when he spoke, he could not steadily govern the pitch of his voice, but it would be sometimes higher, sometimes lower, though, for the most part, what he delivered together he ended in the same key as he began." He could discern in another whether he spoke shrill or low, and he would repeat any hard word after anybody, which the Prince tried often, not only in English, but by making some Welshmen, who served his Highness, speak words of their language, which he so perfectly echoed that "his master himself would acknowledge that the rules of

his art reached not to produce that effect with any certainty, and therefore concluded this in him must spring from other rules he had framed unto himself out of his own attentive observation." The last fact most astounded Sir Kenelme, because, he says, "the Welsh tongue, like the Hebrew, employs guttural letters, and the motion of that part which frames them cannot be seen by the eye otherwise than by the effect they may haply make by consent in other parts of the mouth." Yet this young nobleman read by the motions he observed alone; he could say words after a speaker at a distance when Digby, standing by his side, could not hear them. "But if he were in the dark, or if one turned his face out of his sight, he was capable of nothing one said."

Cure by Sympathy.

The work that has made Sir Kenelme Digby best known in the medical world is his "*Discovery upon the Cure by Sympathy.*" This discovery, so called, was made during his residence in France about 1622-3. On his return he explained the secret of the "cure" to the King, James I., to the Prince of Wales, the Duke of Buckingham, and other well-known persons, and, as it would appear, he brought the "discovery" into practice and gained by the success of it much repute. He did not claim for the secret any original work of his own, but said he had obtained it from a Carmelite friar, who himself had learned it either in America or Persia, rather a wide distinction.

The nature of the sympathetic cure was simple enough; it was a combination of what may be called faith-healing with a process of rest and natural healing. There was called into practice the aid of "a sympathetic

powder," composed of green vitriol, calcined and finely pulverised. This powder was not applied to the body of the wounded person, but to the weapon which had inflicted the wound. The weapon was covered with lard, the sympathetic powder was applied, and then the weapon, the dressing of it complete, was covered up; but the process was sometimes repeated as many as three times a day, the patient knowing all through what was being done for him in this manner. As to the wound, that, brought together neatly with clean linen bandages or strips and covered, according to the prevailing custom, with the admirable friar's balsam, was left to take its rest, without *being disturbed or touched for seven days*, at the end of which time, on the bandages being removed, the wound was usually found entirely and soundly healed.

The practice of the sympathetic cure was considered in its day a marvel. It was translated into English in the year 1658, and it filled its place in the minds of men. As a matter of course, the poets gave a taste of it, for it suited their work of combining the faculty of imagination with the field of fact. In the *Enchanted Island* Dryden makes Ariel give as a direction for treating a wound—

> "Anoint the sword which pierced him with this
> Weapon salve, and wrap it close from air,
> Till I have time to visit it again."

And in another passage he makes believe that the wounded were conscious of the dressing of the weapon; that pain in the wound occurred when the sword was unwrapped, and relief when it was re-dressed and covered from the cold air.

Dr. Paris, in his *Pharmacologia*, to which reference

has been already made, comments with his usual skill on this process of healing by perfect rest through the first intention. He reasoned most properly that "the triumph of the cure was decreed to the mysterious agency of the sympathetic powder which had been so assiduously applied to the weapon, whereas it is hardly necessary to observe that the promptness of the cure depended upon the total exclusion of air from the wound, and upon the sanative operations of nature not having received any disturbance from the officious interference of art; the result, beyond doubt, furnished the first hint, which led surgeons to the improved practice of healing wounds by what is technically called the *first intention.*"

The general truth expressed in the above sentence stands now firmly acknowledged. There is no doubt that wise men soon began to see the true influence that was at work towards cure. In Spain to this day the treatment of wounds by quick closure and avoidance of water has remained the favourite practice, and in France in the last century the distinguished Belloste, in his treatise on the cure of wounds by the exclusion of air from them, established a principle in surgical art that found its way into our own country and became a procedure which, departed from for a time in some quarters, has lately been revived, as if it were a new discovery. We must not, however, entirely disregard the influence which Sir Kenelme Digby denominated as "sympathy." In his day what we call superstition had a much stronger hold on the mind than it has now, and he himself is criticised for that he gave as a cure for warts, "Wash your hands in an empty basin into which the moon shines." Wherever such sentiments prevail faith is strong. There was

some faith, therefore, in the sympathetic cure, and that may have added to the good effect. The treatment of the sword or weapon with the sympathetic powder kept the patient from thinking of the wound, and, above all, from meddling with it. It was cure by diversion of mind, not sympathy, and diversion of mind is truly magical in its influence, as we all know when we recognise the good effect of change of scene and change of occupation during a holiday season. It may be inferred that Sir Kenelme Digby, a man given to the mysterious, was all mystery. Not at all! He was as practical as he was mystical, a fact which will be shown in the next few paragraphs.

The Closet for Cookery.

Amongst his other learned studies, Sir Kenelme had a taste for cookery, and in 1669, by his son's consent, a posthumous work of his was published by H. Brome, at the Star in Little Britain, entitled "*The Closet of the Eminently Learned Sir Kenelme Digbie, Kt., Opened, whereby is discovered Several Ways of Making Metheglin, Sider, Cherry Wine, etc., together with Excellent Directions for Cookery, as also for Preserving, Conserving, Candying, etc.*"

No name is given of the collator of these writings, but, whoever it was, he was very delighted with his task and proud of his author. "The like," he says, "has not in every particular appeared in the English tongue." Metheglin, mead, or meath, seems to have been a favourite drink with Sir Kenelme. He gives for it receipts numerous, and sings his praises over them all. It was a drink made with honey and treated with hops, various herbs, ginger, cloves, and other substances, according to the flavour required. White

mead was much commended, and to one specimen there was given the name "liquor of life." After receipts for meath there are some for country wines and for possets, followed by others for potages, in which are several useful forms. Amongst these potages is one called "Portugal broth, as it was made for the Queen," and appended to this is given the "Queen's ordinary *bouillon de santé*," a dish of which she partook in the morning. It is worth copying in full, as an indication of a royal breakfast meal in the seventeenth century:—

"A hen, a handful of parsley, a sprig of thyme, three of spearmint, a little balm, half a great onion, a little pepper and salt, and a clove, as much water as would cover the hen, and this boiled to less than a pint for one good porringerful."

"Tea with eggs" was a favourite meal in an emergency. "The Jesuite that came from China ann. 1664 told Mr. Waller that they use tea sometimes in this manner: 'To near a pint of the infusion of tea take two yolks of new-laid eggs and beat them well with fine sugar; pour your tea upon the eggs and sugar, and stir them well together. So drink it hot.' 'This when you come home from attending business abroad, and are very hungry and yet have not conveniency to eat, is a competent meal. This presently discusseth and satisfieth all rawness and indigence of the stomach, flyeth suddenly over the whole body and into the veins, and strengtheneth exceedingly, and preserves one a good while from necessity of eating.' It is a fault to let the hot water remain too long on the tea: 'The hot water is to remain no longer than whiles you can say the Miserere Psalm leisurely. Then pour it upon the sugar and eggs. Thus you have only the spiritual

parts of the tea, which is much more active, penetrative, and friendly to nature.' One dram of tea will serve for a pint of water." To all of which I say, Good Sir Kenelme, you were an advanced tea-maker!

Pap of Oatmeal.

Here is another excellent form of food, which I have tried and found as pleasant as it is sustaining. He calls it oatmeal pap or potage:—

"Beat out oatmeal small; put a little of it to milk, and let it boil stewingly till you see that the milk begins to thicken with it. Then strain the milk from the oatmeal and boil the milk to the height of pap, which sweeten with a little sugar, and put to it some yolks of eggs dissolved in rose or orange-flower water, and let it mittoner a while upon the chafing dish. Add a little butter if you like it. You may boil a little mace in the milk."

Gruel of Oatmeal and Rice.

"Doctor Pridion ordered my Lord Cornwallis for his chief diet in his looseness the following grewel: Take two parts of oatmeal and one part of rice in fine powder. Boil these well in water, as you make water grewel, adding a good proportion of cinnamon, to boil also in due time. Then strain through a cloth and sweeten to taste."

Clear Jelly of Bran.

"Take two pounds of bran of best wheat, and put it to infuse in a gallon of water during two or three days. Then boil it three or four hours, and presently take it from the fire and strain it through a fine strainer. Pour off the clear water, and boil it up to a jelly, and season to taste."

Buttered Whiting and Eggs.

"Boil whitings with butter sauce. Pick them clean from skin and bones, and mingle them well with the butter; season with salt. Butter some eggs in the best manner, and mingle them with the buttered whitings, and mash them well together. The eggs must not be so many by a good deal as the fish."

The food preparations are all so excellent, and the book from which they are copied is now so rare, I have thought it useful to give the receipts in full. Each one is of service in the treatment of the sick, and if there were space at my command I should add many more of equal, if not of superior, service. They show, all through, the practical bent of mind of their author, and throw a light on the foods of our forefathers which does them credit for far more skill than that we are accustomed to give to them. The foods, however, are chiefly after the French fashion, and show our Gallic neighbours as even two centuries ago far beyond us in culinary art. The queen to whom Sir Kenelme refers was not the Queen of England, but the Dowager Queen and regent of France, by whom he was held in such high estimation.

EPITAPHAL.

There are many more works of Sir Kenelme Digby that might be set before the reader, but as they bear on theology and other topics not medical in their nature, I must pass them by in order to bring briefly the incidents of the life of this remarkable man to a close. Until the end of the reign of Charles I. he remained in France, but on the declaration of the Commonwealth in 1649 he returned to England for the purpose of looking after his estates. He was bluntly

treated by the new Parliament, which liked him not, and commanded him to leave England at once, with the understanding that if he returned again without permission his life as well as his estates would be forfeited. Thereupon he went back to France, was attached to the dowager Queen Henrietta as her Chancellor, and remained away until the Lord Protector Cromwell attained supreme power in 1653. In these four years the good character of Digby, according to some biographers, was not maintained. The Queen sent him on a mission to Pope Innocent X., to whom he "behaved," according to Wood, "so haughtily that the Pontiff took offence." He was also accused at this time of manipulating for his own purposes some of the funds that had been raised on behalf of the poor and distressed Catholics in England. How far this was true it is hard to say, for, as Chancellor to one of the most unscrupulous women that ever lived, it may easily be supposed that no personal profit of his own caused him to apply political funds to purposes for which they were not intended. It is likewise clear that Charles II. passed over his offence, since he nominated him on the first Council of the Royal Society in 1663 as "Chancellor to our dear mother Queen Mary." It is further worthy of note that the Jesuits, ever his foes, never scrupled to make the worst of him.

But now appears a singular passage. After Cromwell was supreme master, Digby not merely returned once again to England, but became actually a friend of the Lord Protector, who allowed him perfect freedom at home, and permission to strive to obtain freedom for all members of the Church of Rome under the rule of the Protectorate. He passed as he liked to France, resided amongst and discussed philosophy with the

learned men of Toulouse, and sent over from there an account of a petrified city in Tripoli, which, published in our own "*Mercurius Politicus*," caused Henry Stubbes to speak of him as "the Pliny of the age for lying." He moved to Montpellier, then a famous seat of learning; read to the Academy of Sciences there his "Discourse on the Cure of Wounds by the Powder of Sympathy"; travelled in Germany; made his way back to Paris in 1660; and after the restoration of the monarchy here took an active part in the foundation of the Royal Society, to which he communicated a paper on the "Vegetation of Plants," and in the affairs of which he continued to be active so long as his health permitted.

Still young in spirit, though getting old in years, Digby settled down for good at a house he possessed in Covent Garden, London. He had been married some years before to Venetia Stanley, daughter of Sir Edward Stanley, of Tongue Castle, in Shropshire, and by her he had two sons: one, Kenelme, who was slain fighting for the King at the battle of St. Neots on July 7th, 1648, and another, John, who survived him and succeeded to the family estates.

In the Covent Garden home the brilliant and handsome philosopher—for, whatever his faults, he was both brilliant and handsome—ended his days. His house became the centre of the learned men of the time, and his library, which had been collected chiefly in France, fell to the Crown on his decease.

The cause of his decease was stone in the bladder. He died from it on June 11th, 1665, and was buried in a vault in which the body of his wife had lain for some years in Christchurch-within-Newgate. The monument over the vault, and copper-gilt bust of Lady Digby, was destroyed by the Great Fire of London.

The true character of Sir Kenelme Digby has not up to the present time been fairly appreciated on either side. Wood, who does not endeavour to hide his faults, is nevertheless warm in his admiration. He speaks of him as "a person handsome and gigantic, in whom nothing was wanting to make him a complete Cavalier. He had," continues the same author, "so graceful an elocution and noble address that, had he been dropped out of the clouds into any part of the world, he would have made himself respected; but the Jesuits, who cared not for him, spoke spitefully and said it was true. He had a great faculty, which proceeded from abundancy of wit and invention, of proposing and reporting matters to the *virtuosi*, especially to the philosophical assembly at Montpellier and to the Royal Society at home." This from a rather blunt and severe critic was considerably to the credit of the scholar, and was surpassed by R. Farrar, who wrote the following epitaph :—

> "Under this tomb the matchless DIGBY lies,
> DIGBY the great, the valiant, and the wise,
> This age's wonder for his noble parts,
> Skilled in six tongues and learned in all the arts,
> Born on the day he died, the 11th of June,
> And that day bravely fought at Scanderoon;
> It's rare that one and the same day should be
> His day of birth, of death, of victory."

Let us, however, neither be led away by Wood nor Farrar; let us take Sir Kenelme Digby as we find him in his works, and on the pure principle, "nothing extenuate nor aught set down in malice," accept him as he was, a man who rose in spite of the evil star under which he was born, became useful, and died in goodwill with good men.

ORIGINAL RESEARCH—EXPERIMENTAL AND INDUCTIVE.

"Dire n'est rien ; faire est tout."—RENAN.

ON SOME EFFECTS OF HYDRAULIC PRESSURE UPON ANIMAL TISSUES, AND ON VITAL HYDRATION.

IN going over the laboratory of Messrs. Evans, of Liverpool, a few weeks ago, I was struck by the ingenious and practical methods by which they express vegetable juices under hydraulic pressure. I saw here a means by which I could make a few experiments I have long wished to carry out in order to ascertain what would be the effect of extreme pressure on animal tissues, especially on muscular and nervous tissues. Mr. Conroy, the chemist to the establishment, was good enough to conduct the research for me, with results which are, I think, of considerable interest.

Experiment Showing Effect of Hydraulic Pressure on Muscular Substance.

The first experiment was on muscular substance. Muscle of beef, four pounds in weight, quite fresh, was placed in the perforated cylinder of the machine, and was subjected to pressure amounting to two and a half tons on the square inch. The muscle proved to be simply incompressible. The smallest quantity of fluid was expressed after long-continued pressure, and there

was no reduction of mass. It was, as Mr. Conroy remarked, like applying the pressure to a mass of caoutchouc, and when the tissue was removed it was of the same consistency as it was before the commencement of the experiment.

Experiment Showing Effect of Hydraulic Pressure on Muscle after Partial Desiccation.

The same quantity of fresh muscle was partly dried at a temperature of 120° Fahr. It was dried until it had lost half its weight by loss of water. In this state it was placed in the cylinder of the hydraulic machine, and again the pressure was put on up to the degree of two and a half tons on the square inch. The result was the same as in the preceding experiment. There was no exudation and no condensation from the pressure.

Experiment Showing Effect of Hydraulic Pressure on Cerebral Substance.

The brains of six sheep removed from the skull immediately after the death of the animals were wrapped in coarse calico, placed in the cylinder of the hydraulic machine, and subjected to the same degree of pressure, namely two and a half tons to the square inch. The effect was the same as with muscle. There was no exudation and no degree of condensation of substance. It was as if the attempt to compress had been made on water itself.

Experiment Showing the Effect of Hydraulic Pressure on Mixed Muscular and Cerebral Substance.

It seemed worth an experiment to test whether brain substance and muscular substance intermixed could be

influenced by pressure. If they could the fact might suggest the occurrence of a physical or physico-chemical change between muscular and nervous matter when brought into intimate communion. The trial was made as follows :—

One part of fresh brain substance from the ox was mixed up with two of fresh muscular substance from the same animal. The mass was then put into the press as before, and pressure to the extent of two tons and a half to the square inch was applied. The result was precisely the same as in the previous experiments. There was no exudation of fluid and no condensation.

Experiment Showing the Effect of Hydraulic Pressure on Muscular Substance Mixed with Oxygenated Blood.

The next question that occurred to me was, What would be the effect if blood charged with oxygen were mixed with muscular or with nervous matter and subjected to pressure? Experiments to answer this question were made as related below.

The same weight of fresh muscular substance from an ox was well admixed with fresh blood from the same animal, the blood having been defibrinated and richly oxygenated by agitation with oxygen gas. The muscle and blood, after being commingled, were submitted to the same degree of pressure as in the other experiments. In this instance a few drachms of blood were expressed after long pressure.

Experiment Showing the Effect of Hydraulic Pressure on Muscular and Nervous Substance Mixed with Blood Charged with Carbonic Acid.

The experiment was repeated with blood charged with

carbonic acid and admixed with the muscular substance. The results were the same. There was a small exudation of blood under the pressure of two and a half tons to the square inch.

Experiment Showing the Effects of Hydraulic Pressure upon Nervous Substance Mixed with Oxygenated Blood.

The same weight of cerebral substance fresh from an ox, and freed of membrane, was admixed with defibrinated oxygenated blood in the proportion of three parts of cerebral matter to one of blood. Pressure up to the same degree was applied without obtaining any transudation of water from the mass. The mass or pulp would pass bodily through network of fine cloth in which parts of it were enfolded, but no water or fluid would transude from it.

Experiment Showing the Effect of Hydraulic Pressure upon Nervous Substance Mixed with Blood Charged with Carbonic Acid.

The above experiment was varied by mixing defibrinated blood that had been charged with carbonic acid with cerebral substance in like proportions, and submitting the mass to pressure. The results were precisely the same. The mass passed through meshes of cloth bodily, but there was no extrusion of fluid.

Experiment Showing the Effect of Hydraulic Pressure on Muscular Substance Mixed with Sodium Chloride.

Fresh muscular substance from the ox, four pounds in weight, was intimately mixed with four ounces of common salt, and was then subjected to the same hydraulic pressure. The effect was entirely different.

A reddish fluid percolated easily through the cylinder, and eighteen ounces of fluid were extracted without difficulty, leaving behind a compact mass of tough partially dried or compressed muscular fibre. The fluid or muscular juice contained 10 per cent. of albumen, and, on filtration, allowed almost all the saline substance, with some of the albumen, to pass through the filter.

Notes on Vital Hydration.

We may infer from the above experimental results that in their natural state the muscular and nervous tissues are, like water itself, practically incompressible and rigid; that is to say, those tissues are hydrated until they are free of risk of compression by any power of pressure which the force of the circulation can put upon them.

It would seem from this research, as so far carried on, that the only mode in which the brain in its closed cavity of the skull could be influenced by pressure of the blood would be by irregularity of pressure. I mean that if the uniformity of pressure were broken, as by the rupture of a cerebral vessel, then, under interruption of even pressure at the injured part, natural function might be locally disturbed, but not elsewhere.

The results indicate the peculiar character of hydration of colloid substances. The late Professor Graham told me that in his view the hydration of a colloid ought to be looked upon, physically, as a simple process of dilution. No doubt this interpretation is a true one; and yet, strangely, the very dilution may be accepted as a process of solidification of water itself. A firm portion of jelly-fish which weighed ten thousand grains yielded me on complete desiccation a little under ten grains of

solid material. A similar kind of relationship obtains in the semi-solid masses of fibrin which are removed, in some cases, from the heart and great blood-vessels. The puzzle is how water can solidify into firm, colloidal matter, so as to constitute mass. Hydration is not saturation, like the saturation of a sponge; it is a veritable combination, and yet by evaporation the water can be withdrawn. It is peculiar, too, that when colloids have become hydrated and have rendered water semi-solid they can, by some contractile act of their own, make water exude from them, although under such intense compression as we have now seen there is no exudation.

In order for hydration of animal colloid to take place during life by mere surface contact with water, some certain destruction of vital action must be declared. A good observation touching on this point has been made by Dr. Barr in his description of the effect of long continuance of the living body in a water-bath. He tells us that after many days of such immersion there is no effect on the skin, except on the palms of the hands and soles of the feet, which parts, having previously become horny, are rendered soft and sodden, which means hydrated. In like manner, albumen circulating in the blood does not fix water in semi-solid mould or form until it is separated as fibrin. Then it hydrates and yields the plug, and, maybe, it yields the solid structure of brain and of muscle. Perhaps, therefore, all colloid parts die before they hydrate and are moulded into organs in the form of solidified water. Perhaps also many morbid growths are no more and no less than abnormal materials undergoing hydration. Colloid cancer would form a typical manifestation of such an abnormal process; so would a cyst from the walls of which there

was no transudation outwards, but in which there was persistent hydration with internal accumulation. The act of hydration is so simple, it appears to be nothing at first sight. We breathe on a bundle of hairs in the hygrometer, and instantly there is a movement of the needle as an effect of hydration—we commonly say, absorption of water. We could hydrate those hairs into jelly; and if we let them dry they would become of the same weight as when we started with them. If they had vital power of contraction in themselves, they would cause water to exude by particular contraction; but put together and placed under direct pressure, as in the effort to compress water, there is no effect: they and the water are, so to speak, one. This is action of water on dead tissue. If the tissue were living in all its parts, its contraction, in its minute parts, would cause a current, and in connection with evaporation would produce condensation by loss of water. To use a common expression, it would dry out; but it could not be wrung out.

Presumably, then, the most absolute death or suspension of life is that condition in which there is the most complete desiccation; but there is clearly some natural mode by which colloidal substances undergo hydration which we do not understand. It looks as if an organic colloidal substance must die before it can undergo hydration, and yet we cannot make dead structures hydrate after they have vitally and finally contracted. I have tried in many ways to make dried fibrin hydrate, but without success; whilst albumen, which has never been coagulated, will hydrate. Yes, it will be said, because albumen is soluble; but that does not unfold the difficulty. It does not explain the contraction that will expel water in mass, nor why, when the

water is expelled from a tissue by its contraction, hydration does not return.

We see sometimes, oftentimes, good results from natural methods we do not comprehend, and so we see in the hydration of muscular and nervous matter and in the retention of hydration under varying pressures grand results. A nerve in perfect hydration is like a water wire; the brain in its closed cavity, hydrating from the blood and, under tension, acting as a water power, is an accumulation of centres, each centre hydrating independently, giving tension to the nerve that connects it with the body, and keeping muscle in condition for contraction and relaxation, activity and inertia, under the will.

The hydration of the human membranes must be one of the most active of all vital processes, and the cessation of it one of the most destructive. The cessation of that hydration is probably the cause of the rigidity of the tympanum; of the opacity of the lens, in cataract; of the shrinking of cartilages, membranes, muscles, and nervous structures in old age.

Saline substances dehydrate colloids, fix the water they remove, and bear it away if opportunity be allowed for the water, charged with the saline, to escape. So if the aqueous crystalline lens be fed with blood surcharged with saline matter, it becomes opaque and cataractic so long as the saline current is maintained, but gradually becomes clear again as the dehydrating saline dialyses away. This was completely demonstrated in my paper on the "Synthesis of Cataract" in 1860. I found, for instance, that by sufficient addition of very soluble salts, like urea and nitrite of ammonia, to albumen, I could cause albumen itself to dialyse with the saline through a membrane. I found, in like

manner, what Gulliver in another line of research had previously detected: that the addition of a sufficiency of saline to blood recently drawn suspended the vital contractility of the fibrin for long periods of time; but that by diluting the saline with more water the fibrin would rehydrate and then solidify, that is to say, become colloidal in character.

The last experiments, in which the effects were shown of addition of saline matter to colloidal matter, are perhaps most important of all. They indicate the value of saline substance in excretion; they indicate a service which has not before been considered in relation to excretion and in relation specially to the value of the salt of the urine, urea. *In the absence of urea, there could be no excretion of urine. Conversely, in the pressure of an excess of urea or of any soluble salt, such as sugar, there must be an excess of excretion.* Urea is the salt that takes up the water from the colloidal substance of the kidney, fixes the water, and escapes with it. Reduce the natural quantity of urea, and, in proportion, the power of the pressure of the circulation to excrete urine is reduced. Increase the quantity of urea above the normal, and albumen will pass over with the excretion, and albuminoid urea will charge the blood. But I open up here a field of research too important for the mere ending of a chapter, however new: a new chapter of physiology and pathology, for a future date.

COTEMPORARY PRACTICE AND LITERATURE.

"Every physician will and ought to make observations from his own experience; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together."—FRIEND.

"THE TREATMENT OF 'TYPHOID' FEVER." *

[BY JAMES BARR, M.D.]

THE most severe critic who may carefully study this book will admit, if he have a grain of honesty in his nature, that it is a book of mark. The critic may discover that, as the work of a man of bold and ardent nature, the pages have been put together hastily, have not been written with much deference to rules of art, and have not been arranged with any attempt at effect. All this may be found, and yet the finding need not mean fault-finding of a serious character. For a busy man like Dr. Barr there is not much time for refinement of the gold he may, with infinite pains, extract from the furnace of research; sufficient is it for him to draw out any portion of true metal, and give us a present of it fresh from his store.

I remember the time when "typhoid" was not distinguished by the majority of practitioners from other acute fevers of continued type. Practitioners were accustomed in those days to speak of the danger in any

* H. K. Lewis, London, 1892.

serious case of the symptoms assuming the "typhoid type." Many febrile diseases were confounded the one with the other, and typhus and typhoid were nearly always confounded. On this side the Atlantic the distinctive characters of the disease now known specifically as typhoid were first recognised in Glasgow, not by Dr. Patrick Stewart, as Dr. Barr says, but by Stewart's clinical teacher, Dr. Perry, an old military surgeon, if my memory does not fail me, who, having retired from the military service of his country, devoted the last days of his useful life to the treatment of the fever-stricken in the fever wards of the Royal Infirmary of Glasgow. I recall him well to mind—an aged and reverend man, with a gentle smile and word for every one, and a real love for those who became his pupils and followed his clinique. I recall his first words to me as I began to study under him: "So you are not afraid, my dear boy, to go round with me? There is some risk, of course, but you must not fear it." Perry knew the special disease now called "typhoid." He knew that it was not contagious in the same way as typhus and remittent fevers were, and he sent some patients suffering from it into the general wards. He called the disease, not typhoid, but "*dothinenteritis*," a much better, though not a very manageable, word. Who was so foolish as to stamp the specific disease "typhoid" instead of "enteric" fever, its true designation, I do not know. For my part, I have always abided by its true name—*enteric fever*—and I trust Dr. Barr in the next edition of his work, which will soon be wanted, will follow in the same path.

Dr. Barr's treatise gives us a short history of the discoveries that have been made in respect to the fever and the course of it in those whom it afflicts. He

adds also a description of fifty-five successive cases treated by a particular method, with only one death. Each of these examples is a special study, and is presented as if the patient were brought before the eye of the reader. The symptoms are explained, with the variations they present in different persons under differing circumstances; and altogether this part is a clinical gallery through which, as he traverses it, the student cannot fail, if he read diligently, to learn lessons of a practical kind which alone are worth a month of labour. It is not, however, to this section of the work, necessary and incidental as it is, that the student is specifically invited to attend; the grand object is to show one particular mode of treatment, "not a specific treatment for the disease, but a specific treatment for the patient." I have always taught students, under my tuition, that the essence of sound treatment consists in striving to place the sick in such conditions that they cannot die. Dr. Barr, in his treatment of enteric fever, seems to have held this method steadily in view.

The treatment itself, in its leading character, consists in placing the patient in a water-bath below the natural temperature of the body and keeping him in that reduced temperature, not for an hour or so, but for a period of several days, amounting in some instances to weeks, so that the temperature of the whole of the body may without violence be brought down to the normal from the febrile abnormal, and kept there until the acute symptoms have altogether passed away. Of the steps by which the author moved to the adoption of this practice we are clearly informed, and it does great honour to his candour that he gives the fullest credit to those who have preceded him in their attempts to treat the disease by the application of cold to the

body. Particularly, he does full justice to his forerunner, also a physician of Liverpool, Dr. Currie, who many years ago, in a bold, scientific, and successful method, employed cold affusion for the treatment of acute fever, and who was amongst the pioneers in the use of the thermometer in the study of febrile conditions. Currie, in short, approached extremely near to the establishment of a correct system, and led the mind of his successors, and of his present successor most signally, to a more rational and, I may say, practical development of what, if it be continued by others with the same results as have been obtained by our author, promises to become one of the simplest, the most certain, and the most remarkable advancements in practical medicine of the century, for the management of one particular form of disease.

The theory upon which the practice is based is that the danger incident to the febrile state consists rather in the physical injuries produced by the increment—we might well call it the unearned increment—of heat on the tissues than in the effect of the added ferment by which the febrile condition is lighted up, and by which it is sustained. In plain language, the fabric of the living house is being injured by the heat to which it is subjected more than by the material that is being burned to maintain the heat. It would, as a matter of course, be the best practice of all, if it could be done, to adopt some plan that should immediately put out the excess of fire or fever, and radically stop the fever at one stroke; some day this will be done. At present we do not know of such a decisive plan, but we do know that if time can be gained the fire will come down to the normal of itself, and if at that point of natural decline the body shall remain in a condition

fitted for recovery, recovery will follow under proper precautions. What then, if we can succeed in keeping down the dangerous changes induced purely by the febrile heat until the normal conditions return, will be the state of the body for recovery at the end of the required time? The answer, as derived from both physiological and clinical learning, is that the conditions for recovery will be better if the heat resulting from the intensified zymosis be kept down by cold. It is true, there will have been a continuous waste of energy, but dehydration will have been subdued, and the fatal molecular changes that are due to persistent exposure to heat and dehydration will have been less destructive than if the fever heat had been allowed to run its unchecked course.

This point admitted, the next question is: What is the most correct mode of keeping the fever heat low without reaction and the shocks of reaction, that are invariably prejudicial? The idea which ran in our author's mind was that it would be the best policy not to restrain by a sudden abstraction of bodily heat, which might give rise to extreme reaction or to collapse, but to abstract gently and persistently. He pursued, therefore, the following course: he placed his febrile patients from enteric fever in a water-bath at a temperature not lower than eight degrees Fahr. under the natural temperature of the body, and kept them there long enough for the temperature of the body to be brought down near to or at the normal, feeding, meanwhile, being properly attended to, and all other practices of a medicinal character prescribed for, as occasion might require. The construction of the bath was such as made it like a bed, in which the patient could repose on a couch of bed-tick, with the head

raised on a shelf of the same material carrying an air cushion. The body was covered with the water, which could readily be changed, and, indeed, if required, be kept in steady current of change; whilst over the bath was thrown a waterproof counterpane, supported by a wooden cover, by which the water was prevented from cooling, and the appearance of an ordinary bed was, to some extent, secured. The patient was wrapped up in a blanket and completely immersed except the head. "We have never found it necessary," says Dr. Barr, "to lower the temperature of the bath below 90° Fahr. nor raise it above 98°. As long as the patient's temperature in the mouth is over 100°, the temperature of the tank need not rise above 90° to 93°; but as the body temperature approaches the normal, so should the tank temperature." Plain water seems to have been used at first, but later on some aseptic substances were introduced, which do not appear to have answered well. "Perhaps," Dr. Barr suggests, "the skin of the patient might be protected by a coating of some mineral fat, and then a strong antiseptic might be used." This is one of the very few indifferent suggestions in the book: fat as a non-conductor would interfere with the eliminating function of the skin, and would make an objectionable puddle.

The reader must gather the details for himself from the work in hand. It is only possible here in a few lines more to notice some of the leading results. Under the tank treatment there was exhibited a true antipyretic action; the "thermogenesis" was diminished, and the "thermotaxic mechanism" improved. There was marked improvement in the vaso-motor tone. The respirations lessened in frequency, and bronchitis and congestion of the lungs decreased and soon disappeared.

The improvement in the digestive system was perhaps more marked than anything else, but the delirium disappeared, and the general well-being of the patients greatly improved. There was marked diminution in the dehydration of the tissues which takes place in all febrile conditions, a fact very apparent from the tongue, which maintained its proper size and remained moist.

The most satisfactory results from Dr. Barr's mode of treatment have reference to the reduced mortality following upon its employment. In the middle of this century we did not think one death in four from enteric fever at all peculiar; and down to quite recent times one death in ten has been considered a wonderful result. But Dr. Barr has had an experience of fifty-five successive cases with one death, and since then one other death with sixteen recoveries, so that he now stands at two deaths in seventy-one cases—by far the lowest mortality that has yet been reached, perhaps the lowest to be expected until we arrive at the radical mode of directly quenching the febrile fire, without endangering normal combustion.

A warm-hearted introduction to this essay, written under peculiar circumstances by Professor Gairdner, of Glasgow, assigns to Dr. Barr "the credit of an experiment at once bold and cautious, on a sufficient scale to command attention, and with results that cannot easily be explained away." It would be very difficult to find choicer or wiser words with which to bring to a close this review, and I am content to repeat them, with my best hopes that the experiment will have the fullest and fairest trial, and that the results of the future will confirm to the letter the results of the past.

"THE ERUPTIVE AND CONTINUED FEVERS: A TEXT-BOOK." *

[BY JOHN WILLIAM MOORE, M.D.]

TO pass from the book of Dr. Barr to this by Dr. Moore is like passing from the room of a medical Stoic to the reception of a brilliant and versatile medical host, who is ready to introduce his guests to six or seven hundred men of different standards of mental quality, each one having said something somewhere respecting Eruptive and Continued Fevers, and each one being more or less known, personally or by repute, to the accomplished entertainer. The book of Dr. Moore is called a text-book. It is a volume of five hundred and thirty-five closely printed pages; and it has this value as a text-book: that although simply and clearly written from beginning to end, it is so well filled with modern teaching that any student who can master it may snap his learned fingers at any examiner who may tackle him on the subjects here put before him. The intimate nature of fever, the intimate nature of contagion, micro-parasitic diseases, the general principles of treatment in the eruptive and continued fevers, the exanthemata or eruptive fevers, the continued fevers, infection and immunity,—these, divided into forty-nine chapters, make up the volume, which, as a student still, I have carefully read, marked, learned, and inwardly digested, with interest always, often with profit, yet not with equal satisfaction.

Whenever Dr. Moore is speaking from his own knowledge and experience in this volume, he is sound as a nut; but when he departs from that line, he is not so sound. True, he supplies from others

* Fannin and Co., Dublin, 1892.

only the fashionable doctrines of the schools, and that what he gives is given classically and with a scholarly tone that brings back Mason Good himself to the mind of the reader; true also that for students who are wanting a text-book for examinational trials it may have been necessary to put forward every word that is written; yet this only distinguishes the more decidedly the speculative from the actual, and shows more the error of connecting the speculative with the actual. The germ hypothesis of disease saturates the whole volume, as a practical working exposition of cause, of effect, and, largely, of practice. The student has no escape from the dogma that "the pathogenic organism of the infective diseases is specific—that is, it is peculiar to the particular disease, standing by a causal relation to it and to no other malady. Thus the microbe of scarlet fever gives rise to scarlet fever, and to it alone, that of small-pox to small-pox, and to it alone. 'The seed, as a gardener would say, comes up true.' Hence these maladies are spoken of as *specific* or essential febrile diseases. This is an established clinical and pathological fact." Sometimes it happens that there is no microbe. In the volatile poison of typhus there is no known microbe; for influenza, which sweeps down a province at a blow, no microbe has been found. How then, we poor students ask, can such diseases occur and yield no microbic sign? The answer comes, no microbe has been found, but there *must be one*. The whole fabric of the germ speculation would fall down like a house of cards and be heard of no more if this answer were not ready.

Dr. Moore's work shows, with brilliant lucidity, the curious psychological fact that when once an idea has become fashionable everything that is said in favour

of it becomes gospel. The most shadowy assertions or assumptions, by any one whatever, go for accepted truths, without question. No one, whoever he may be, can write a line in favour of the germ speculation without being accepted as an authority. On the other side, no one can venture to say a word against its most flagrant absurdities without being branded as behind the time, or as prejudiced, or as ignorant, or as obstinate. If a person were wicked enough to invent, absolutely, a set of statements or arguments in sustainment of the speculation, with just such a clever touch of controversy in them as to create a little excitement, he could not fail to stand out, in a moment, as a new and original inquirer. Who he was would never be asked; what his abilities were, what were his facilities for research, where he lived, or by what authority he spoke, would never for a moment be questioned. He might adduce evidence of having performed work which, faithfully reckoned up, would require from an ordinary idle person like myself, for instance, a hundred years of labour, and yet everything he said might pass without challenge, or being challenged, might only be challenged in regard to some minor detail, and then as if it were from beginning to end *bonâ fide*.

The student who masters the speculative parts of Dr. Moore's book will find, or will naturally infer from the reading of it, that when an eruptive fever is established it has been established because there has been set up a "zymosis." The student will wonder. He will ask, How does the increase of animal heat in the fever differ from the natural heat that is always produced within the body? Is this fever a mere exaltation of the natural fire, or is it a new fire, altogether, added to the old one?

He will naturally ask, What is the origin of the natural animal fire itself? Is it, too, a zymosis; and if so, what is its microbe? But if the animal natural fire goes on always without a microbe, goes on by some other process, chemical or chemico-physical, why in fever should it not be that the fever is a mere exaltation of the natural combustion? It is not the student's duty to ask such questions. He must believe; he must not question. He must believe implicitly every experiment that supports the dogma. A disease has broken out for which there is no explanation the student can discover. He inquires why such an attack should be. The reply is ready: the disease sprang up because the victim of it came, by accident, into contact with "a resting spore," which might have been lying by fifty or a hundred years, ready at any moment to germinate into a microbe when brought into conditions favourable for its development. But has any one lived long enough to prove such a series of events from a spore resting for so long a period in harmless oblivion? That has nothing to do with the matter; a resting spore is a resting spore, or it would not have been so called.

It is painful to see so able a writer as Dr. Moore obliged to tie himself hand and foot to a speculation which may, without the least imagination, take the turns above suggested; which may, after all, be founded merely on dangerous analogies and natural coincidences; which ignores the nervous origins of diseased conditions; which fails to explain the commonest phenomena of the most obvious diseases, like influenza; which, as Dr. Creighton has shown, becomes "mere verbalism" when applied to the explanation of the leading facts of history; which fails whenever it is turned to practical account on the grand scale that could alone prove its

validity; which leads men from the simple and open paths of prevention into subtleties of useless character; which allows any pretender to rank as an authority who will merely rig out his ridiculously leaky tub to run with the flowing tide; and which, in one instance at least, has exposed medicine to the merited insult of the cheated vulgar. It is grateful to find him in other parts in his true sphere, an accomplished and learned physician, giving us his own knowledge and tempering it with that wisdom which planes and squares and fits it into shape. Like Murchison, to whom he refers about one hundred and sixty times, he is truly at home when he is on the clinical platform. To him, in that true place, we students, free from the idols of the den, are listening, in the light of day, to a teacher descended from another teacher, to whom he so affectionately dedicates his volume, and whose honoured name all of us who remember it love to recall with respect and admiration.

COLD BATH TREATMENT IN TYPHOID AND PNEUMONIA.

[BY GEORGE WILKINS, M.D.]

SINCE writing the last two notices I have studied a paper, by Dr. George Wilkins of Montreal, published in the *Medical News* (Lea Brothers, Philadelphia) for May 28th, 1892 (pp. 394—600). It was read at the meeting of the Association of American Physicians held in Washington on May 24th, 25th, and 26th. Dr. Wilkins uses the bath by what he calls Brand's method. Whenever the temperature of the rectum reaches 102·2°, without regard to the time of day, he places the patient in water at a temperature of 68° Fahr., keeping him there for fifteen minutes, making cold applications to the head and using friction

over the body. By this method, and the occasional use of antipyretics, he has been able to reduce his mortality to 7·3 per cent. in cases of enteric fever.

Dr. Wilkins' experience extends to 39 well-marked cases of enteric fever with 37 recoveries. The immediate effect of the bath was an average reduction of temperature of 1° Fahr. in one case; 2° in two cases; between 2° and 3° in nine cases; 3° and 4° in twenty-six cases; and over 4° in one case. In 95 per cent. of the occasions on which the bath was used the pulse fell within half an hour after the patient was removed from the bath sometimes only 2 or 3 beats, but usually more; on some occasions as many as 18 beats.

The almost universal rule was that the patient rested quietly after the bath, when previously he had been restless; and in those cases in which notes were taken regarding sleep, it was found that in most of them there had been sound and refreshing sleep, probably of short duration, but always invigorating. Dr. Wilkins admits that, although the refreshing effects of the bath are usually present, it is very rare indeed to find any patients who like the treatment, and in one instance a patient who was delirious resisted the treatment so violently that it had to be discontinued.

Dr. Wilkins inclines to the idea that enteric fever may be aborted by the bath. He is not yet convinced on the point, but he is hopeful. In support of this hopeful view he refers to a series of cases in one family, in *one* of which the fever did seem to have been cut short by the bath.

The influence of the bath appears to have been favourable in respect to complications. Tendency to hæmorrhage was not increased, and pulmonary complications were less frequent; instead of the patient "taking cold"

from the bath, "quite the reverse is the case." "The respiratory troubles arise from a weak heart. The degenerating heart muscle pumps blood with enfeebled power into both systems; the impaired vaso-motor activity manifests itself in the lungs in a twofold manner: it affects both the bronchial and the pulmonary vessels. Usually both sides of the heart are involved in the respiratory troubles, producing more or less complete broncho-pneumonia." So little, in fact, does this author fear pulmonary troubles from the use of the bath, that he has treated successfully a case of pneumonia itself by cold baths alone. In *resumé* he adds: "It is a fact that cannot be denied, that in about 90 per cent. of conditions of pyrexia in which the cold bath is systematically used, it will reduce the temperature, lessen the pulse rate and frequency of respiration, and increase the daily amount of urine secreted, besides promoting if not actually causing sleep."

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APPENDIX OF ACKNOWLEDGMENTS.

ADLER. "The Health Laws of the Bible and their Influence upon the Life-condition of the Jews." By MARCUS N. ADLER, M.A., F.S.S. London. 1892.

ALLEN. "Five Years' Public Health Work." By FRANCIS J. ALLEN, M.D. London. 1892.

BARTON. "Tables for the Diagnosis and Treatment of Syphilis." By JOHN KELLOCK BARTON, M.D. Dublin: Fannin & Co. London: Baillière, Tindall & Cox, King William Street. 1892.

BATEMAN. "On Asphasia." By SIR FREDERIC BATEMAN, M.D. London: J. & A. Churchill. 1891.

BENNETT. "Marlborough in Neolithic Times." By F. J. BENNETT, F.G.S., H.M. Geological Survey. 1892.

BLACKWELL. "Erroneous Method in Medical Education." By E. BLACKWELL, M.D. London: Women's Printing Society. 1891.

CAINE. "Address to the Electors of the East Division of Bradford." By WILLIAM S. CAINE. London. 1892.

COHEN. "A Case of Tracheotomy for Multiple Neoplasms of the Larynx." By J. SOLIS-COHEN, M.D.

——— "Laryngology." Philadelphia. 1892.

COLLINS. "Traumatic Hydronephrosis." By W. J. COLLINS, M.S. London. London. 1892.

COOKE. "Dissection Guides." By THOMAS COOKE, F.R.C.S. London: Longmans, Green & Co. 1892. [A little work of the most unpretentious character, but to the anatomical student an invaluable guide in his practical work.]

CORMACK. "The French Riviera—Hyères as a Health Resort." By CH. E. CORMACK, M.D. Philadelphia: W. B. Saunders. 1891.

COSGRAVE. "The Control of Inebriates." By E. MACDOWEL COSGRAVE, M.D. Fannin & Co. 1892.

CURGENVEN. "The Disinfection of Scarlet Fever and other Infectious Diseases by Antiseptic Inunction." By J. BRENDON CURGENVEN, M.R.C.S. London: H. K. Lewis, 136, Gower Street. 1892.

DIXON AND ZUILL. "Reaction of the Amide Group upon the Wasting Animal Economy." By PROFESSOR SAMUEL G. DIXON, M.D., and W. S. ZUILL, D.V.S. Philadelphia: American Medical Press Company, Limited. 1892.

DOW. "Maine Liquor Law." By the HON. NEAL DOW. Manchester: United Kingdom Alliance, 16, Deansgate. 1892.

EISENBERG and PIERCE. "Bacteriological Diagnosis. Tabular Aids for use in Practical Work." By JAMES EISENBERG, M.D. Translated by NOVAL H. PIERCE, M.D., Philadelphia and London. F. A. Davis & Co., 1892.

FINLAYSON. "Galen." A biographical demonstration.

——— "Celsus." A biographical demonstration.

——— "Hippocrates." A biographical demonstration. By J. FINLAYSON, M.D. Glasgow: Alex. Macdougall, 1892. [Three learned and admirable discourses.]

GOODHART. "The Harveian Lectures on Common Neuroses." By JAMES FREDERICK GOODHART, M.D. London: H. K. Lewis, 136, Gower Street, W.C. 1892.

HAZELL. "Hazell's Annual for 1892." London: Hazell, Watson & Viney. 1892. [This annual continues to grow more useful as it grows in age. The 1891 epitome of English life.]

HICKS. "The Church and the Drink." By the REV. E. L. HICKS, M.A. Manchester: United Kingdom Alliance. 1892.

JONES, JOSEPH. "Observations on Koch's Lymph."

——— "United Confederate Veterans." By JOSEPH JONES, M.D., Surgeon-General's Office, U.C.V., 156, Washington Avenue, New Orleans. Louisiana. 1892.

KLEIN. "A Night among the Infinities." By SYDNEY T. KLEIN, F.L.S., F.R.A.S. Middlesex Natural History Society. 1892.

KNAPP. "Accidents from the Electric Current." By PHILIP COOMBS KNAPP, M.D. Boston: Damrell & Upham. 1892.

KYLE. "The Pathology and Treatment of Tetanus." By D. BRADEN KYLE, M.D. S. Davis. 1892.

LANE. "Differentiation in Rheumatic Diseases." By HUGH LANE, L.R.C.P., M.R.C.S. London: J. & A. Churchill, 11, New Burlington Street.

LOWE, CHARLES. "Four National Exhibitions in London and their Organiser." By CHARLES LOWE, M.A. London: T. Fisher Unwin, Paternoster Square.

LOWE, JOHN. "A List of Fishes known to Occur in the Norfolk Waters."

——— "On Silicified Vegetable Structures from the Zambesi."

——— "On Sarcina Ventriculi."

——— "Wayside Botany and Notes in Norway." By JOHN LOWE, M.D. London.

LOWNDES. "Reasons why the Office of Coroner should be held by a Member of the Medical Profession." By FREDERICK W. LOWNDES, M.R.C.S. Eng. London: J. & A. Churchill. 1890.

McKENDRICK. "Spallanzani." An address at the opening of the Medical Session of the University of Glasgow. 1891.

——— "Some Problems of Modern Physiology." President's address to the Philosophical Society of Glasgow. 1890-91.

——— "Human Muscle as a Transformer of Energy." President's address to the Philosophical Society of Glasgow. 1891-92.

——— "Chronological Tables of Scientific Men." By JOHN G. McKENDRICK, M.D., LL.D., F.R.S. Glasgow: Maclehose & Sons. 1892.

MARSH. "St. Paul's Cross." By JOHN B. MARSH. London: Raithby, Lawrence & Co., Limited. 1891.

MITCHELL. "Opening Address to the Institute of Hygiene of the University of Pennsylvania." By S. WEIR MITCHELL, M.D., LL.D. Harv. University of Pennsylvania Press. 1892. [A brilliant address, full of fact as of suggestion.]

MORLEY. "The Direct Popular Veto." Speech on the renewal of licences for the sale of intoxicating liquors. By the RIGHT HON. JOHN MORLEY, M.P. London. 1892.

QUAIN. "Quain's Elements of Anatomy." Edited by E. A. SCHAEFER, R.R.S., and G. D. THANE. Vol. II. Part II. London: Longmans, Green & Co. 1892.

REMONDINO. "The Mediterranean Shores of America. Southern California: its Climatic, Physical, and Meteorological Conditions." By P. C. REMONDINO, M.D. Philadelphia and London: F. A. Pains & Co. 1892.

SCHAEFER. "The Essentials of Histology." By E. A. SCHAEFER, F.R.S. London: Longmans, Green & Co. 1892.

SELTMAN. "On Stammering." By E. J. SELTMAN, B.A. London: Hansard Publishing Union. 1891.

SEMPLE. "Elements of Materia Medica and Therapeutics." By C. E. ARMAND SEMPLE, M.B., M.R.C.P. London. London: Longmans, Green & Co. 1892.

SISLEY. "Influenza and the Laws of England concerning Infectious Diseases." By RICHARD SISLEY, M.D. London and New York: Longmans, Green & Co. 1892.

SQUIRE. "Shakespeare and English History." By WILLIAM SQUIRE, M.D. Ealing Broadway. 1892.

STEAVENSON AND JONES. "Medical Electricity." By W. E. STEAVENSON, M.D., and H. LEWIS JONES, M.D. London: H. K. Lewis, 136, Gower Street. 1892.

STEWART. "Our Temperaments." By ALEXANDER STEWART, F.R.C.S. London: Crosby Lockwood & Son. 1892. [A new edition, with some finely executed illustrative plates.]

TARNER. "Detonation in Meteorology." By GEORGE EDWARD TARNER. London. 1892.

TUKE. "Chapters on the History of the Insane in the British Isles." By DANIEL HACK TUKE, M.D., F.R.C.P. London: Kegan Paul, Trench & Co., 1, Paternoster Square. 1892.

WALDIE. "The True Story of the Introduction of Chloroform into Anæsthetics." By DAVID WALDIE; and Remarks by GEORGE WALDIE. Edinburgh: Oliver & Boyd. 1870.

WALMSLEY. "Outlines of Insanity." By FRANCIS H. WALMSLEY, M.D. London: The Scientific Press, Limited, 140, Strand. 1892.

WILLIAMS. "Woodhall Spa." By C. J. WILLIAMS, L.R.C.P. London: Bemrose & Sons, 23, Old Bailey, E.C. 1892.

WILLIAMS. "The Medical Annual." By P. WATSON WILLIAMS. Bristol: John Wright & Co., Stone Bridge.

WILSON. "Micro-organism of Diphtheria, with Experimental Results in Animals." By ALBERT WILSON, M.D. London: Adlard & Son. 1892.

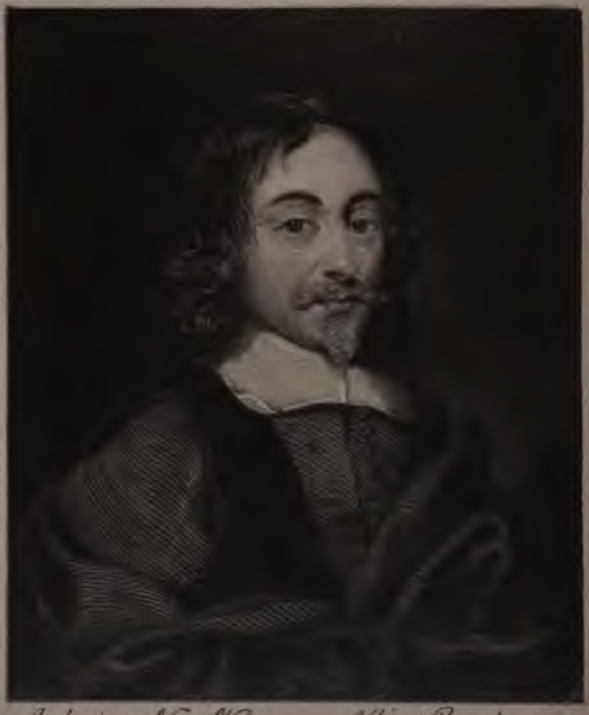
REPORTS, PROCEEDINGS, AND TRANSACTIONS.

"Transactions of the Epidemiological Society of London." Vol. X. London: Shaw & Sons, Fetter Lane, E.C.—"Transactions of the Sanitary Institute." Vol. XII. London: Offices of the Sanitary Institute, 74A, Margaret Street, W.; Edward Stanford, 26, 27, Cockspur Street, Charing Cross, S.W.—"Transactions of the West London Medico-chirurgical Society." Volume IV. Edited by P. S. ABRAHAM, M.D., and R. W. LLOYD. London: Baillière, Tindall & Cox, 20 and 21, King William Street, Strand, Paris, and Madrid.—"The Thirty-fourth Annual Report of the Ladies' Sanitary Association." London: Office of the Ladies' Sanitary Association, 22, Berners Street, Oxford Street.—"Twenty-seventh Annual Report of the National Temperance Society." New

York: National Temperance Society and Publication House, 58, Reade Street. "Report of the Homes for Inebriates Association and Eighth Annual Report of the Dalrymple Home at Rickmansworth." London: H. K. Lewis, 136, Gower Street, W.C.—"Transactions of the Cremation Society of England." 8, New Cavendish Street, Portland Place, W.—"Annual Report of the Ealing Microscopical and Natural History Society." *Times* Printing Works, Broadway, Ealing.—"Temporary Home for Lost and Starving Dogs, London." Battersea Park Road, South Lambeth, S.W.—"Borough of Margate." Annual Report of the Medical Officer of Health and Meteorological Report for the Year 1891. *Keele's Gazette* Office, Margate.—"The Vestry of the Parish of St. Leonard, Shoreditch." London: A. T. Roberts & Son, 5, Hackney Road, N.E.—"Toynbee Hall." Seventh Annual Report of the Universities' Settlement in East London. London: Penny & Hall, 53, Leman Street.—"Brief Comments on the Pathology of Pneumonia." By N. S. DAVIS, M.D. Chicago: R. R. McCabe & Co. 1892.—"The Wills Eye Hospital." Reports for the Years ending 1890 and 1891.—"Arterio-capillary Fibrosis." By AUGUSTUS A. ESHNER, M.D. Philadelphia. 1891.—"Some Principles Involved in the Nature and Treatment of Inebriety." By T. L. WRIGHT, M.D., Bellefontaine. Ohio. 1892.—"The Social Centres of London." London: Polytechnic Reception Bureau. 1892.—"Monism, its Scope and Import." Chicago. 1891-92.

EXCHANGES.

"The New York Medical Journal." "Journal of Chemical Society." "Journal of Society of Arts." "Chicago Med. Journal and Examiner." "New Orleans Med. and Surg. Journal." "Sacramento Med. Times." "Illustrated Science Monthly." "New England Med. Monthly." "Chemical News." "College and Clinical Record." "Glasgow Med. Journal." "Bristol Med. Chir. Journal." "Australasian Med. Gazette." "Journal of Mental Science." "Quarterly Journal of Inebriety." "Sanitary Journal." "Hygiene Pratique." "Med. Temperance Journal." "The Polyclinic." "Asylum Journal." "Journal of Microscopy." "Longmans' Magazine." "Gentleman's Magazine." "Belgravia." "Chemist and Druggist." "Health." "Sanitary World." "Journal d'Hygiène." "Malthusian." "Animal World." "Scientific American." "Med. Record." "Bulletin de l'Académie de Médecine." "Liverpool Med. Chir. Journal." "Medico-legal Journal." "Hospital Gazette." "Journal Scottish Meteorological Society." "International Journal of the Med. Sciences." "Journal of Heredity." "The Health Journal." "Science Gossip." "The Med. World." "The Photographic News." "Edinburgh Med. Journal." "The Wayfarer." "South African Med. Journal." "Phrenological Magazine." "Med. and Professional Review." "Vegetarian Messenger." "The Analyst." "The China Med. Missionary Journal." Shanghai. "Séances et Travaux de l'Académie des Sciences." No. 5. Par JULES SIMON. Paris: 1887. "The St. Louis Med. and Surg. Journal." "The Journal." "The Canada Health Journal." "The School Board Chronicle." "Our Day." "The Scientific Inquirer." "The Monist." "Public Health." "The Satellite." "The Band of Hope Journal." "University Med. Magazine." "The Journal of Physiology." "New York Abstract." "Murray's Magazine." "Journal of the American Med. Association." "The London Figaro." "Provincial Med. Journal." "Albany Med. Annals." "Accidental Med. Times." "Diet and Hygiene." "The Open Court." "Journal of Physiology." "Journal of Nervous and Mental Disease." "Pictorial World." "Horticultural Times." Med. Analectic and Epitome." "Journal of Dermatology." "The Med. Record." Good Health." "Med. News." "Journal of British Dental Association." "American Journal of Med. Sciences." "Drug Topics." "Abkari, Indian Temperance Journal." "Woman." "The Central Review." "Rivista Internationale d'Igiene." "Australian Med. Gazette." "Boy's Own Paper." "University Med. Magazine." "The Friend of China." "Provincial Med. Times." "The Canine Journal." "The Tocsin." "The Hospital." "The Gentlewoman." "The Author."



Asclepiad No. 30 Vol. 8. Third Quarter 1892.

Tho. Browne

Autotype from an engraving, for Wilkin, 1836; by Edwards, from White's engraving in first folio edition of Browne's collected works, 1686. and from original picture at Oxford.

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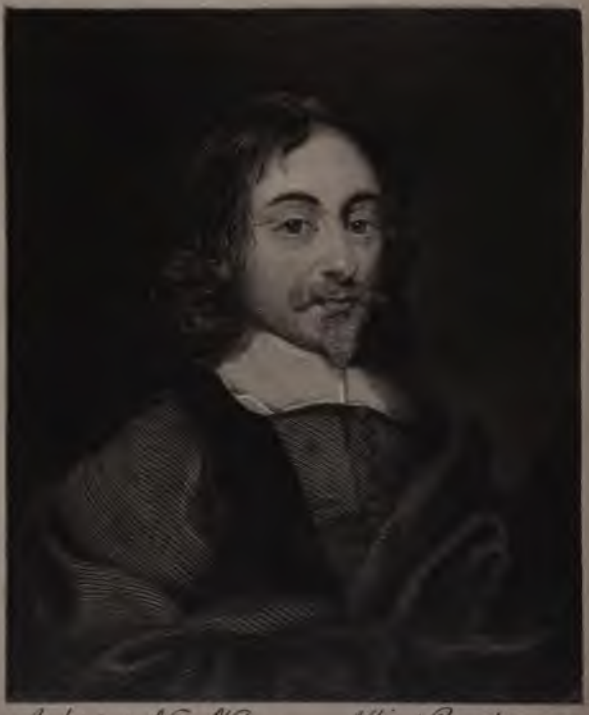

Asclepiad No. 33 Vol IX. Third Quarter 1892.

Tho - Browne

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BMR

ON THE CAUSE AND PREVENTION OF DEATH FROM CHLOROFORM.

*LECTURE III.**

THE IMMEDIATE PHENOMENA OF DEATH BY OR UNDER CHLOROFORM.

IN the present lecture I bring forward, in the first place, for consideration the important subject of the immediate cause or causes of death occurring during the administration of chloroform. We have discussed the rate of death from the administration, the conditions of the patient physical and mental leading to death, the state of the surroundings of the patient, the mode of administration, and the quality of the chloroform itself; but we have not yet studied the immediate phenomena of death from chloroform when that event occurs, nor the cause or causes by which those phenomena are brought about. These are the tasks now before us.

MODES OF DEATH.

There are four modes of death from chloroform: (1) *by apnœal syncope;* (2) *by epileptiform syncope;* (3) *by cardiac paralysis;* (4) *by shock.*

* Delivered April 5th, 1870.

recommences to contract, the ventricle follows rhythmically, and in time all the motion of the organ is restored.

The human subject sinks, as we know, from this mode of death under chloroform, and we can understand now the kind of subject that is most likely to succumb in the manner described. It will be the young or feeble subject or the irritable, nervous, shrinking subject, such a one as would be likely to faint under sudden shock or alarm; or it will be the vehemently resisting subject, one who, excited by the first impression of the vapour, would grow violent under coercion or restraint while made to inhale the vapour. The remark by Dr. Sansom will recur to us here that persons who are in health and are about to be subjected to some minor operation are put under chloroform with more difficulty than are those who, by long illness and suffering, are resigned to their fate, and who, prepared to meet the worst, have, to some extent, lost the sense of fear.

I cannot, while treating of this rapid mode of death, avoid saying a word against the promotion of it by an error in administration of chloroform which may be called a moral error—an error most reprehensible, cruel, insane, and yet not uncommon. This is the error: to bring a patient just prior to the performance of an operation, when the anxiety which weakens the strongest man or woman is filling the mind—to bring the patient thus circumstanced, with all the senses open, into the presence of an assembly of practitioners and students and commence without any preparation, or soothing quieting expression, the administration of the narcotic vapour. It is not a wonder that some persons die under this ordeal; the wonder is that so many survive it. To bring a living intelligent human being

to the borderland of life by any narcotic is not a fool's errand for mankind, but a solemn service, and I think in all cases of death from narcotic gases and vapours this point relative to administration is one on which a coroner might make due inquiry, with advantage to the profession not less than to the community.*

DEATH BY EPILEPTIFORM SYNCOPE.

The second mode of death may be called death from muscular excitability—*epileptiform syncope.* It occurs usually during the second stage of narcotism—the rigid stage, as it has been designated. In this stage the chloroform is acting as an excitant to the whole muscular system through the blood, which conveys it over the organism, and in this excitement the voluntary as well as the involuntary muscular fibres share. Efforts to vomit not unfrequently precede death during this period of administration, and the death if it happen is instant, and by pure and simple syncope. If we could suddenly empty the whole arterial system of blood, a more perfect syncope could scarcely be induced, and, in fact, the emptying of the arterial system is what really takes place, for if we open the body of an inferior animal immediately after this form of death, we find the left side of the heart with the arteries empty, and the lungs entirely blanched and bloodless, bloodless as if they had been washed out with water by injection. I have shown this condition, as you will remember,

* An eminent American physician lately here expressed to me a few days ago that nothing, during his medical observations in this country, surprised and pained him so much as the error I have ventured, in the interests of all, so freely to condemn.

many times. When, again, we turn to the structure of the brain, we find it in the same blanched and bloodless state. All through the body, in short, there is evidence afforded, on the arterial side of the circulation, of intense arterial contraction. The venous side tells in another way the same story, for the right side of the heart is engorged with blood, the veins are engorged, the sinuses are engorged. The arteries, in fact, have poured their contents into the veins, and contracting firmly in their minute ramifications, have shut up the blood in the veins and produced complete arrest of motion throughout the circulation. Considering how commonly and how severely the second stage or degree of narcotism under chloroform is developed, it is a matter of wonder that death is not more frequent during this stage. Happily, if the vapour is administered steadily and freely, the stage of excitement is comparatively short, and, happily also, when the next or third degree of narcotism is reached, the danger of continued muscular rigidity is past. When death occurs in the second degree of narcotism from chloroform, the rigidity of muscle alternates with relaxation, the result being a convulsive effort resembling true epilepsy.

Why in exceptional cases death should occur during the stage of muscular excitement, while so many persons pass through the ordeal of that stage unscathed, I cannot readily answer. The cause of death is excessive irritation of the sympathetic nervous system, and it is fair to assume that any previously deranged condition of that system would be a reason for the danger. Any pre-existing irritation of the stomach or of the intestinal surface would be a predisposing cause of danger, as would prolonged emotional excitement and hysteria.

The fatal accident might happen, when the excitement is extreme, from indecision on the part of the administrator as to whether he should stop the administration altogether, or push it on to the production of the third degree. In the inferior animals, death is readily induced by mere prolongation of the second stage, and what is true in respect to the lower animals is, I think, equally true in respect to man.

DEATH BY CARDIAC PARALYSIS.

The third mode of death from chloroform is one in which, from the slow and continued action of the narcotic, there is *paralysis of the heart*, with apparent paralysis of the muscular system generally. If we take a perfectly healthy animal, narcotise it carefully, and bring it into what was called by Snow the fourth stage, or the stage of complete muscular relaxation, we shall have led the animal up to the border of death; if we then carry our narcotism a little further, the heart and the respiration will cease together, but before the heart shall cease it will give us intimation it is about to stop by one important sign: it will give an *intermittent* stroke. See, here are two animals in the profound and all but fatal fourth degree of narcotic sleep. One animal is a pigeon, the other a rabbit. We remove the bird from the vapour, and observe how complete is the muscular relaxation; we listen to its heart and hear how distinctly that organ is proclaiming, by its intermittent pulsation, the imminent approach to the cessation of its labours. It is like a stopping watch, which gives a few ticks, then waits, then goes on again. But now that the vapour is no longer being inhaled, and that the oxidation is unimpeded, the recovery of

the animal is certain. We will let the other animal remain in the vapour, and soon it ceases, but almost imperceptibly ceases, to breathe and to circulate blood. We say it is dead, and so it is, but this I would point out: it is not equally dead throughout its body. Its voluntary muscles respond to galvanism, the respiratory muscles respond, but when we expose the heart and subject it to the galvanic stimulus it is absolutely dead.

This mode of death from chloroform would very often happen if in every operation it were necessary to carry the insensibility to the fourth degree. Fortunately this is not the case, for under chloroform the consciousness fails before the failure of the muscular power, and when there is unconsciousness we can in most cases proceed to operation at once; thus we rarely run the risk of the extreme degree of narcotism, or if we reach it we withdraw the vapour; and, the subject before us being healthy, we secure the recovery, in the same way as we have secured it in the bird which has been narcotised to-day.

DEATH BY SHOCK.

The fourth mode of death is a compound death; there are in it two factors: *depression from chloroform and surgical shock.* The combination may be in two or three ways. In a few instances of death, hæmorrhage has brought to a fatal degree a depression which had commenced during and from the administration of chloroform. The death here is by syncope, and is often sudden. In other cases the patient has not been fully narcotised, but in a half-unconscious state feeling the pain of the operation, has become faint and has died from syncope. In this case, again, the death is sudden. Dr. Marshall, of Mortlake, has particularly directed my

attention to this mode of death, and has dwelt, I think, with force and truth on the errors sometimes committed, first in supposing, in the case of small operations, that it is only necessary to administer a little narcotic vapour, and secondly in proceeding to operate while the patient is excited and not insensible. There have been several deaths at this juncture of the first step of an operation during the first degree of narcotism. Finally, under very deep narcotism, carried near to or into the fourth degree of insensibility, death may take place from severity of shock incident to operative procedure. I have noticed always in administering an anæsthetic to deep anæsthesia during the performance of the operation of ovariotomy that when the operator first puts his hand into the peritoneum, or when he uses the sponge to clear out the abdominal cavity, there is an alteration in the beat of the heart, a quickness of beat with feebleness, and even with intermittency. I remember also a case where, in order to break up firm adhesions in the knee-joint, I administered chloroform to a lady to the degree of complete muscular prostration, while Mr. William Adams made sharp and forcible bending of the joint. With each act of the operator in this case, the pulse stopped for the moment as if it had been mechanically arrested, and then after a pause recommenced its beat. It was necessary on three separate occasions to perform the operation thus named, and on each occasion the phenomenon described was equally well marked.

The case of death under chloroform which recently occurred in Scotland, when Sir James Simpson administered the vapour, was, I conceive, an instance in which the fatal termination was due to the combination of narcotism and surgical shock.

IMMEDIATE CAUSES OF DEATH.

When we have clearly before us the modes of death from chloroform, we are led the more readily to see the immediate cause or causes of death, and in this survey we strengthen our view if we combine with mode of death the phenomena which naturally accompany ordinary narcotism under chloroform.

In the first stage of administration, the effect of the vapour is upon the peripheral nervous surface and the cerebral surfaces. In both there is excitation, and very early the cerebral centres lose their natural condition, becoming suspended in function. Following immediately upon this, the cord, the sympathetic system, and the true nervous system of the heart, become excited. Thirdly, the nervous excitation ceases generally, there follows calmness, or even depression of action, and if the administration be continued the medulla fails, the sympathetic fails, the cardiac nervous centres fail. This, then, is the natural order of death of each part: brain; cord; sympathetic centres; cardiac centres.

It does not appear that the act of producing cerebral unconsciousness is ever a cause of danger to life, and, except in cases where the vapour is too irritating, we have little evidence to show that during the administration of chloroform there is any primary risk from injury done to the centres of the nervous system that govern respiration. The danger really commences when the vagus or the organic nervous centres, or both, become implicated, for then the contractile elements of the vascular system are at once modified in function. Wonderful light has been thrown on cardiac action during the last few years, and no subject does that

light more illuminate than this we have now specially in hand. In his excellent paper, to which I referred at the last lecture, Dr. Rutherford sustains the modern theory "that the contractile elements of the entire vascular system are presided over by two kinds of nerves, one motor, the other inhibitory," the motor bringing about "contraction," the inhibitory throwing "the motor nerves and contractile elements into a state of rest." The term "inhibitory" is, I fear, unfortunate as conveying the idea of an active process, which is not such, but a passive process. I will not at this moment, however, quarrel with a term, since it is more important to treat of the theory in relation to the action of chloroform, and the theory tells us simply: (*a*) that if the vagus be subjected to irritation the motion of the heart is arrested quickly, but that after a time it may recommence to beat even when the excitation is continued; (*b*) that paralysis or division of the vagus increases for a time the speed of the heart; (*c*) that the two cardiac nervous centres with branches from the sympathetic are the real motor nerves of the heart, and, (*d*) that between the two sets of nerves, the inferior cardiac branches of the vagi and the motor centres, there is, in the natural state, a balance of action.

In all the modes of death from chloroform we see a fatal disturbance of this balance. In the first mode—by syncopal apnœa—we see the direct action (inhibitory, so called) of the pneumogastric upon the heart; in the second mode—by epileptiform syncope—we see the direct effect of the excitation of the centres which supply the motor or contractile elements of the vascular system; in the third mode—by failure of cardiac and muscular motion—we see the effect of the poisonous

agent upon the motor system carried to paralysis; and in the fourth mode, when surgical shock combines with depression from chloroform, we see the direct paralysing action both on the sympathetic and pneumogastric nerves.

After death by chloroform it is the common practice to report on the condition of the heart of the patient. Thus sometimes the heart is said to have undergone fatty degeneration, at other times to be thin, and, again, to be quite healthy, so that from the many and different reports in different fatal cases respecting the condition of the heart no rule can be deduced which the morbid anatomist can accept as explanatory of death, or on which, in other cases, the diagnostician can rely as indicating danger of death. In this there is nothing remarkable. Let us bear in our minds the truth that chloroform, when it kills, kills not necessarily by its action on the muscular structure of the heart, but by its influence on the nervous mechanism of the heart, and then the actual condition of the muscular structure becomes secondary in estimation.

In conclusion, I infer that in every case of death from chloroform the cause of death is excitation either of the motor or of the controlling nervous mechanism of the heart. I conceive that any primary organic changes of structure leading to death are situated in that mechanism, and must be looked for there; and I think there is fair ground to assume that in some cases there may be death where there is no actual disease of structure, but simply so extreme a natural delicacy of balance between the nervous functions that the excitation produced by the chloroform is sufficient to arrest motion and to destroy life.

PREVENTION OF DEATH UNDER CHLOROFORM.

PREPARATIONS FOR ADMINISTRATION.

We are brought now to consider the all-important point, the prevention of death under chloroform. We have seen that a certain small number of persons seem constituted to die from chloroform, and a certain large number constituted to recover from its effects. Those constituted to recover would recover, however the vapour were administered, unless the most unreasoning and reckless plans were followed in administration. Those predisposed to die would die even when fair and, it may be, capable administration is carried out. But the large majority recover, and consequently there is a tendency to administer alike to all, as if, in truth, all things, including even the effects of chloroform, were alike to all. It soon becomes a belief that it is impossible to make any exceptional rules for administration. There is no fixed rule for determining why exceptional precautions should be taken; why then make exceptions which only lead to perplexity or confusion? This is the argument.

I agree that there ought to be no exceptional rules. What ought to be is simple. Every person to whom chloroform is administered, every person healthy or unhealthy, good subject or bad subject, should be submitted to chloroform as if he or she were the worst subject for it that could possibly come under the hand of the administrator.

The preparations for administration are of three kinds: those which pertain to the surroundings of the patient; those which relate to his or her personal comforts and conditions; those which belong to mode of administration.

THE SURROUNDINGS OF THE PATIENT.

1. *Repose.*

In the preparation of the patient for chloroform there ought to be no hurry, no excitement, on the part of any one. Very many of the deaths that occur are connected with hurry and worry. The patient may go to the hospital or consulting room having no idea beforehand that chloroform is likely to be administered. Suddenly the communication is made that chloroform is necessary. This, to persons of nervous and susceptible nature, is most serious. Belief in the necessity has to contend with doubt, and doubt engenders fear. In this troubled state, with the heart in perturbation, the patient unwillingly and fearfully submits to fate, submits in very bad condition for the ordeal; submits with a fainting heart, and, having a fainting heart at the best of times is ready to die from slight oppression. In a fatal case that recently occurred in a private operating room the anxiety expressed by the operator is that he too hastily insisted on the administration. "The patient died as much from fear and from flurry as from the chloroform," was his sad explanation to me. "It was all over in a minute, like a syncope from a fright or start, but it was a fatal syncope."

One patient of this class whom I knew was fortunately of stronger will. The administration was suddenly pressed upon her. She asked, "Is there not some danger in this process? Might I not die, in my nervous state?" "Some danger, but not much," was the reply. "Then I will not risk any, for I am not prepared to die." The resolve was final, and no chloroform was administered, but the strain of the ordeal was such that a paroxysm of what might be called

hysterical syncope supervened, which of itself gave alarm, although it ended in recovery. Had chloroform been administered, the chances are as ten to one, at least, the syncope had been fatal.

It is not too much to urge that all hurry should be avoided in the administration of this dangerous remedy. There should be no fear on the part of the patient, no hesitation, no excitement. Whilst administering, too, there should be repose and quiet, like that which is sought when we go to court natural sleep in bed. I have already said that to take a human being into an operating theatre and in the noise and bustle of a large class of lookers-on to administer chloroform is unpardonable. I repeat the statement without offence and appeal to the sound judgment of all my compeers whether there ought to be any offence in naming a matter so important. Once only in my career have I taken upon myself to administer chloroform in the presence of a large class, and I would not do the act again under any circumstances. It was as unfair to my means of observation as it was to the safety of the patient under my care.

2. *Food and Feeding before Administration.*

In considering the preparation of the patient for chloroform, a word should be said about food. Experience has taught unmistakably that chloroform should never be administered to a patient whose stomach is charged with food. Persons so circumstanced usually vomit, and, moreover, are readily disposed to become faint. On the other hand, it is very bad practice to administer chloroform to a person exhausted from long deprivation of food. An interval of two hours is sufficient after food, and a longer interval is, I think,

injurious if the food last taken be carefully selected. The best food that can be given before administration is, for an adult, a breakfast cup of beef-tea or milk, or of tea with an egg beaten up in it, and charged, in either case, with grated toast or biscuit.

3. *State of the Alimentary Canal.*

I have not yet said all about the preparatory treatment. As it is bad practice to administer chloroform when the stomach is loaded with food, so it is equally bad when the bowels are loaded with fæces, or when they and the stomach are distended with gas. The surgeon should never allow a patient to be put under chloroform without previous inquiry as to the condition of the bowels, or without, if he finds them loaded, the pre-administration of a gentle purgative. A few grains of grey powder and scammony, followed by an aloetic draught, a day previous to the administration of chloroform, is a most valuable preparative for those in whom the diaphragm is impeded in its free action by constipation and flatulent oppression.

4. *Use of Alcohol.*

Opinions vary as to the propriety of giving alcohol immediately before administration. If we could put a proper quantity of alcohol into the blood before commencing the administration, the shock from introduction of chloroform would be moderated. The practice is *morally* good, and when a nervous patient is pacified by having it, a glass of sound wine should not be withheld.

5. *Freedom of Respiration.*

It may seem childish to insist that a person under chloroform should enjoy the most perfect muscular

freedom, so that the breathing may be naturally performed and the circulation left unimpeded. In plain truth, it is not childish to enforce these rules, for they are constantly broken. In some operations, as in removal of the jaw, some surgeons still carry out the plan—necessary before the days of anæsthesia, but worse than unnecessary now—of binding the upper limbs of the patient to the trunk. In other cases tight clothing is allowed to envelop the trunk of the patient, and frequently many clothes are allowed to rest on the abdominal muscles. Further, it is no uncommon practice when, in the second degree of narcotism, the patient struggles, to use force to restrain movement, instead of pushing on the chloroform to the third, quiescent, and much safer degree of narcotism. All these proceedings, negligences, or practices, are essentially dangerous; they are not trifles. It should be ever remembered that the living body under chloroform is a body more than three parts dead, and that such power as still remains in the organs all-essential to life is so much reduced that the smallest untoward circumstances may turn the scale towards death. Perfect freedom for respiratory movement is, consequently, a vital preparation for the administration of chloroform.

6. *Position of Body.*

The *position* of the patient has been often dwelt on, and it, too, is important. The sitting position is certainly unfavourable for the heart; the perfectly recumbent position is unfavourable in many ways: it interferes with free respiratory power, it permits fluids accumulated in the mouth to fall back into the throat, it allows the tongue to fall back, and when vomiting

happens to come on it enforces the necessity for raising the body. For all these reasons the semi-recumbent position is the best, as it is the most convenient. To this recommendation as to the position it is essential to add another—viz., to keep the body, from the beginning to the end of the administration, in the same position, without any upward or downward movement. We find in deeply narcotised animals nothing so detrimental as movement of the body. Left at perfect rest, we may narcotise an animal until the inspiratory act of respiration ceases, and yet the animal may recover; but at this crisis the smallest movement, the merest handling of the body, may prevent all chance of return of power. The same rule holds good when an animal is being resuscitated from chloroform. After the first gasp of inspiration proclaims returning life, rest is absolutely essential to successful recovery. Precisely the same rule holds good with the human subject; yet so little is the fact known that when there are symptoms of collapse it is the most common of practices quickly to lift up or quickly to lower the body. The practice seems of little moment; it may be of infinite moment. In saying this, I do not raise objections to the plan of turning the patient once gently over to the left side, as Mr. Bader so strongly recommends, for that, I think, at first is a good and commendable plan. I speak of the raising and lowering of the body, and the exercise upon it of strong and sudden pressure.

7. *Equable Temperature.*

An equable temperature of the air surrounding the patient is of infinite importance. The temperature should be at a mean of 65° Fahr. It should never be

below 60° nor above 70°. If we turn back to our table in which the diffusion of chloroform vapour in atmospheres of different temperatures is given from precise measurements it will be seen at a glance how necessary it is to follow rigidly a correct rule in relation to temperature.

8. *Dryness of Air.*

It is no difficult matter at all to secure the dryness of the air which is required for correct administration, for it is merely necessary to have a good fire in the operating room or theatre when the air is naturally charged with moisture. In operating rooms of hospitals, George's stove, commonly called the "*Calorigen*," would answer the purpose with precision, and the degree of moisture can at any moment be determined by readings of the dry and wet bulb thermometer. The rule as to this reading should be that whenever the dry and wet bulb differ less than five degrees the air is not sufficiently free of moisture. This is a very simple rule, and in hospital practice, at all events, could be carried out with regularity and determination.

These then are the eight preparatory rules for the administration of chloroform :—

1. Repose and freedom from excitement and hurry.
2. Freedom from exhaustion due to want of food.
3. Freedom also from oppression arising from the distension of the stomach from food or from constipation.
4. Administration of a moderate dose of alcohol some minutes previous to inhalation.
5. Freedom of respiration and from bodily restraint.
6. Uniform semi-recumbent position of the body.
7. Uniformity of atmospheric temperature, at a medium of 65° Fahr.

Uniformity of aqueous vapour in the atmosphere, so that the dry and wet bulb of the thermometer do not differ more than five degrees.

Administration.

After what I have said in Lecture II. bearing on rapid and slow administration, it is not necessary to dwell at length on the same subject in this place. The point of practice is to administer at first slowly until the lung becomes accustomed to the irritation and the pulmonary reflex has subsided, then to push on freely so as to prevent the stage of violent muscular action or convulsion and to get the narcotic action with as little expenditure of chloroform and saturation of the system as is possible.

COMMENTARY, 1892.

I.

From the statements related above I have nothing to withdraw. They are all sound to the best of my maturer judgment. I have, however, to add a few suggestions which have occurred to me in the course of the passing period, since 1870.

Two Reflexes under Chloroform.

In 1870 I recognised fully what Rutherford first so ably described: the direct reflex from the action of the vapour of chloroform on the respiratory surface. I have since seen that there is a second reflex due to the absorption of the chloroform vapour by the blood and its distribution over the peripheral surface of the body at large. This is the reflex that leads to the symptoms that mark the second degree of narcotism defined by Snow: the degree in which there is general convulsive action

of the voluntary muscles. It is, in fact, nothing more and nothing less than an echo or extension of the first reflex, but it is more generally distributed, and is attended with a certain measure of unconsciousness. It is, like the primary, a dangerous reflex, and it adds to the dangers of chloroform administration considerably. When the reflex dangers have passed away, and the nervous system has tolerated the shocks sustained, all is smooth, and narcotism can be carried up to the extreme of the fourth degree, up to the very point of death, with comparative freedom from danger. I found that by the direct injection of chloroform into the veins of the lower animals the second reflex could be induced without the primary. The cause of both reflexes is the element chlorine in the chloroform, an element which in proportion to the quantity present is the immediate cause of all danger in bodies of the chlorine series. My object in introducing methylene bichloride was the reduction of this danger by the elimination of one equivalent of the cause of the danger.

II.

ALCOHOLISED CHLOROFORM.

My views as to the practice of administering alcohol previously to the administration of chloroform remain unchanged; and I am still so impressed by its importance, I have in Asclepiad XXXII. made the matter the subject of a special opusculum. I only differ from my suggestion of 1870 as to mode of giving the alcohol. Originally I spoke of administering wine, not being conversant at the time with the utter unreliability of the factitious compounds called wines. I have therefore substituted for wine alcohol itself, so as to ensure precision of action. I recommend either alcohol

pure, or rectified spirit, half-ounce doses of alcohol pure, one ounce of the rectified spirit, to be given well diluted with water before the administration, letting the administration of the chloroform commence when, but not until when, the indication of the relaxing action of alcohol is pronounced. I have not the slightest doubt that this is sound and scientific practice. If a perfectly pure *methylic alcohol* were readily obtainable it might be mixed with chloroform direct in the proportion of one part of methylic alcohol and three parts of chloroform. These fluids would vaporise almost equally and equably, and the combined vapours would yield an unusually safe compound, under which it might not be necessary to administer alcohol by the mouth. The compound might be called, for brevity, *alcoholised chloroform.* I have administered it several times, and have found it most regular and satisfactory in its action.

III.

PURE NARCOTIC DEATH.

Under chloroform, as under all anæsthetic gases and vapours, there is a mode of death which may be called the final or natural. It need never be produced, and never could be except under the most unskilful management, and it is a long time in its progress. When death does occur in this manner, it is by the slow extinction of the natural animal zymosis, and is illustrated, as to method, perfectly by the simple experiment of gradually extinguishing a candle in a confined space by introducing vapour of chloroform into the air that fills the space. It can be illustrated also by the experiment of stopping ordinary fermentation by the presence of chloroform and even by the simpler

process of using chloroform vapour as a preservative of animal tissues from decomposition. All anæsthetics are open to kill in this manner, but that is the safest anæsthetic which puts out life in no other manner, which does not, that is to say, cause either of the reflexes of spasmodic character during administration.

IV.

THE CIRCULATION IN EXTREMIS FROM CHLOROFORM.

I have twice made a careful study of the condition of the circulation during deep narcotism and simulated death under chloroform. In the frog the narcotism can be gently made so deep that all signs of life are absent, and may remain absent for hours, and yet when the apparently dead animal is removed from the vapour and is placed in a warm atmosphere, so that the chloroform can escape from its tissues, life may be completely restored. Observing this curious fact, I availed myself of it in order to watch, microscopically, the circulation in the web of the foot during all the stages of narcotism. I have published details of this research in one of my reports to the British Association for the Advancement of Science, but I may relate in this place that, as the phenomena of simulated death progress, the course of blood through the capillaries ceases until at last the courses of the capillary tubes can no longer be traced. In this condition the structures may remain for periods varying from one even to three hours if the parts be kept moist. Then, if the chloroform vapour be permitted to evaporate, the circulation may return. But—and here is the important fact—when the circulation does return it is always by the venous side. That which is first observed is an ebb of blood from the right heart along the veins back

towards the capillaries. The ebb recedes, returns with increasing force until it may be said to pulsate, and so gradually the radicles fill. Upon this, if recovery be the fact, there follows a faint pulsation from the arterial side as if the heart was pushed into motion by a *vis a tergo*, and at last the circulation is restarted. This has led me to say that the veins are the rivers of life, and it may be that the ancients, after all, were right when they called the liver the centre of life.

V.

APPARATUS FOR ADMINISTRATION.

I have myself administered chloroform by every conceivable method, and I am bound to repeat that given a healthy person who has no fear as to what is going to happen, death from chloroform can only take place, under any method, by sheer carelessness. For this reason some blunderers, who entrust administration to hands however unskilful and to any method however primitive, are able to boast of their success. If they go on long enough they always have fatal results, but they get such long stretches of success, they lose the idea of risk, and it is fair to admit that any fatal catastrophe that did occur might occur whatever method were employed. I do not attach so much importance, therefore, to special apparatus as some do; but if I have a predilection it is for the simple Wolf's bottle apparatus, commonly known as Junker's and first constructed by Dr. Junker for the administration of methylene bichloride. This apparatus is precise in its action and economical in respect to quantity of fluid administered; it is clean and pleasant for inhaling from, and it has this great advantage that the administrator, by proper use of the hand bellows with which

he drives over the vapour, can deliver the vapour in smaller or larger proportions as he may desire.*

VI.

A BALANCED OPERATING TABLE.

In some of my earliest researches, conducted at Mortlake in the years 1850-54 and published in the "*Association Journal*," I noticed that when a lower animal was killed by vapour of chloroform the heart, even after it had ceased to contract, could be made to recontract by simply holding up the body of the animal with the head downwards. Nelaton practised a similar process on the human subject in a case of supposed death from chloroform, and with success. There is a good deal in this practice, and it suggests that the table in the operating theatre of the hospital should be so balanced that by a simple movement it can be raised and lowered so as to bring the head of the patient quickly down and up again in a rocking movement. To some extent this would lead to change of air in the lungs, and artificial respiration, but the grand advantage would be that under the movement the blood on the right side of the heart would be diverted towards the lungs, while that on the left side would be diverted to the brain. Also by the up-and-down movement blood would be stirred in the vessels and in the cardiac cavities, and a friction induced between the blood and the walls of the cavities, which tends powerfully to restore cardiac contraction and propulsion.

* The makers of this apparatus, Messrs. Krohne & Seseman, have lately added to it an ingenious contrivance by which the lightest movements of respiration can be observed from a feather valve *during inhalation.*

*SANTÉ; RICHESSE; BONHEUR.**

DR. PEAN, mon cher ami de Pietra Santa, Messieurs et chers Collègues,—Mes regrets sont sincères de ne pouvoir vous parler dans le vrai langage de cette belle patrie, cette patrie du savoir, cette patrie de la lumière, cette France, la magnifique. Mon œil peut lire vos livres, et mon esprit saisit aisément, et joyeusement, les leçons de votre savoir, de votre art, de votre science, de votre industrie, en un mot de votre génie tout entier! Mais mon oreille anglaise et ma langue anglaise, faute d'habitude, sont inhabiles à saisir la note de votre parole, et à y répondre harmonieusement; faute que vous accepterez, comme vous acceptez, j'en ai la confiance, toute ma bonne volonté.

D'abord, et avant tout, qu'il me soit permis, au nom de mes collègues et au mien, de vous remercier, du fond du cœur, de la générosité vraiment merveilleuse que vous nous avez montrée. On dit parfois que le chemin sûr et certain de gagner le cœur d'un Anglais est d'y arriver en passant par son estomac. Je crois bien que cela n'est pas spécial aux Anglais. Mais je puis vous parler d'un autre chemin privilégié pour arriver à nos

* Response to an address by Dr. Pean, President of the Société Française d'Hygiène, on the occasion of a banquet given by the society to the Sanitary Inspectors' Association at the Grand Hotel, Paris, on Monday, June 6th, 1892; and ventured, "with all its imperfections on its head," as a maiden speech in the French tongue.

cœurs et c'est celui de notre esprit, de notre intelligence. Dans ces jours de progrès incessante, notre esprit réclame aussi impérieusement sa nourriture que le fait notre cœur, et cette nourriture vous nous l'avez donnée si généreusement, que pendant bien des années futures nous pourrons vivre, sur ce capital de forces amassées, ce festin intellectuel.

Messieurs, une légende scientifique nous dit qu'autrefois la France et l'Angleterre ne formaient qu'un seul et même continent. Aucun bras de mer ne nous séparait. Il n'y avait entre nous aucune de ces vagues impétueuses, rugissantes, mugissantes ! Nous nous promenions à travers les mêmes plaines et les mêmes forêts ; nous chassions les mêmes proies ; nous reconnaissions le même gouvernement ; nous parlions la même langue ; nous chantions les mêmes chansons ; nous poursuivions les mêmes amours ; nous n'étions qu'une seule et même nation, qu'un seul et même peuple. Puis, vint la Mort, la mort pétrifiant tout, de sa main glacée. Alors apparut la période glaciale, des siècles s'amoncelant aux siècles ; et quand cette période s'évanouit, il se trouva que le profond canal avait été creusé entre nous, lequel canal nous a séparés pendant des périodes séculaires, et il se trouva que l'Angleterre avait été laissée isolée sur son roc escarpé, baignée dans ses flots, comme une veuve baignée dans ses pleurs ! Au lieu d'une nation, deux nations rivales, souvent en guerre pour les raisons les plus puériles, parfois pour les raisons les plus cruelles, presque toujours pour les raisons les plus incompatibles.

Enfin, espérons que cette rivalité est ensevelie pour toujours dans la nuit des temps, que l'époque glaciale et ses conséquences ont cessé pour toujours, ainsi que cette période d'animosité et ce frottement mortel de

jalousie nationale, d'orgueil, d'égoïsme, qui a fait plus de mal dans cette petite planète que l'éclair, que l'avalanche, et l'orage, que Dieu nous envoie et nous donne le pouvoir de nous maîtriser à mesure que nous avançons dans l'étude de ses divines lois ! De là vient qu'aujourd'hui nous qui sommes ici, nous renouvelons les jours de l'âge ancien et primitif, et nous nous tenons ici comme sur notre Terre Patrie.

Et au nom de quelle cause sommes-nous ici ? Nous sommes ici au nom de la Santé. Et comme la santé nationale est la richesse nationale, nous sommes ici au nom de la plus riche, de la plus heureuse de toutes les causes pour laquelle il soit donné à l'homme de travailler sous le soleil. La terre entière possédant la santé, la terre entière possédant la richesse, la terre entière possédant le bonheur. *Santé ! Richesse ! Bonheur !* telle est la devise flottant sur notre bannière. Les anges eux-mêmes peuvent-ils aspirer plus haut ?

A vous donc, Messieurs, nos plus sincères remerciements : à vous dans le cœur de qui a germé la pensée de jeter aujourd'hui la semence de la victoire ; semence qui doit pousser, fleurir et fructifier, quand nos deux nations s'unissent dans leur grandeur, comme dans notre vraie simplicité, nous nous unissons dans cette Société d'Hygiène ce soir !

Et avec l'expression de notre ardente reconnaissance, nous, Anglais de santé, nous nous écrions : Vivent la France et l'Angleterre unies et indivisibles, combattant pour la délivrance universelle de l'humanité ; délivrance de la misère, délivrance de la maladie, délivrance de la mort !

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."
BENJAMIN RUSH.

NERVOUS AND MENTAL DERANGEMENTS FROM ORGANIC SULPHUR COMPOUNDS.

IN one of his valued science jottings in the *Illustrated London News* (July 30th, 1892) Dr. Andrew Wilson refers to two instances in which peculiar derangements of mind were induced in workmen who had been exposed to the inhalation of the sulphur compound known as *sulphuretted hydrogen.* The facts related on the observation of Dr. Wrigglesworth, of the Rainhill Asylum, recall some original observations of mine rendered in a report to the British Association for the Advancement of Science in 1870. I had introduced by this time an entirely new line of research in therapeutics, namely that of studying the modification of effects produced by chemical substances from the introduction or abstraction of simple elementary parts, and had pointed out that the correct line of research was "to commence with a base and to follow the modifications of its action through the varied compounds formed upon it." In this manner I had studied the action of the series of bodies called alcohols, and had indicated with much care the peculiar action of the substance known as *mercaptan* or *sulphur alcohol*, a fluid made by saturating an alcoholic solution of potassa with sulphuretted hydrogen and then

treating the solution with ethyl iodide. I showed that a person brought under the action of this sulphur compound was subjected to extreme variations of mind and body, namely, a desire for sleep, a strange, unhappy, dreamy sensation as from some actual or impending trouble, succeeded by an easy but extreme sense of muscular fatigue, so that the limbs felt too heavy to be lifted, with depression and slowness of the pulse, effects which last for several hours, until, in fact, the sulphur is eliminated. I also observed the fact, very important to remember in respect to the relationship of muscular paralysis and mental disturbance, that, while other narcotic and paralysing agents cause primary paralysis of the voluntary muscles and secondary paralysis of the heart and the respiratory muscles, with recovery—when recovery occurs—first of the voluntary and afterwards of the involuntary; under mercaptan the voluntary muscles lose their irritability last and regain it first.

These were facts I felt worthy of notice in respect to mercaptan, but another special and new fact I described was that this and other sulphur compounds might readily be constructed in the living animal economy, and that there was opened to physicians a new field of inquiry as to the presence of sulphur compounds in the air expired by the sick and in their other excretions. I argued that in disease the breaking up of the albuminous textures is often attended by the formation within the body of volatile sulphuretted organic compounds, and that the detection of such compounds would prove a most useful aid in diagnosis. When we know how minute a proportion of sulphur alcohol will produce mental depression bordering on suicidal propensity, we may infer that the formation of

sulphur compounds *within* the body would account for many examples of excessive temporary prostration, for the cause of which we have as yet no explanation.

In a further research on the same subject I came to the conclusion that the influence exerted over nervous matter by the element sulphur, in disintegration, was so marked in mental aberration that it was inevitable that melancholia and other nervous affections, attended with or without paralysis of voluntary muscles, must some day be accepted as due to the presence of compounds of this element; that there was abundant evidence from odour alone of the presence of mercaptan in the excretions from the lungs, the skin, and the alimentary canal; and, that by inquiry in this direction we had before us the first steps towards a rational explanation of insane conditions produced as deliriums from intoxication by intoxicants developed in the vital organic chemistry. In respect to deliriums from sulphur products so many corroborative facts have now been recorded in addition to these latest by Dr. Wrigglesworth, the view I advanced in the report may be considered as a demonstration.

Dr. Andrew Wilson is always so just to my work, I would not for a moment impute to him a word of neglect on his part towards me. I am sure he has simply forgotten or by accident overlooked this bit of history of practical discovery from physiological labour. Nor do I wish—although I might be proud of the results—to recall the labour from any personal ambition. I am simply anxious that in days to come—in which I cannot tell his story—when physicians are diagnosing and successfully treating diseases of nervous character, at present incurable and inscrutable, from studying the excretion of sulphur compounds from the odies of

their patients, the credit of the method may be honestly accorded to the English school of medicine of the nineteenth century.

METHYLENE FOR INTERNAL ADMINISTRATION.

METHYLENE bichloride, commonly called methylene, a substance made by the reduction of chloroform under the action of zinc, and which I brought out as a general anæsthetic in the year 1865, has up to the present time been employed only as an anæsthetic. I wish now to intimate that it has many properties which commend it for administration in solution alone or in combination. Methylene is a little more soluble in water than chloroform, is pleasanter to the taste, and blends more easily with other medicinal fluids. The dose of it runs from five minims to thirty, and in the largest of these doses it is agreeable to take when freely diluted with water. In action it is antiseptic, slightly stimulant, antispasmodic, and anodyne. Its antiseptic properties are remarkable, and, in combination with peroxide of hydrogen, it forms perhaps the best of all antiseptic combinations. In typhoid I believe it to be by far the best. The prescription runs as follows:—

℞ *Methyleni Bichloridi*, ʒj.; *Solut. Hydrogenii Peroxid.* (10 *volumes*), ʒj.; *Acid Hydrochlori Diluti*, ♏ xxx.; *Aquæ Destillatæ*, *ad* ʒvj. *Fiat Mistura. A twelfth part to be taken in half a tumbler of pure water every three hours or as directed.*

This mixture may be used by the patient as if it were a simple drink. It does not interfere with the action of food; the tongue becomes clean under it, and the pyrexia falls. The same solution may also be

administered, in typhoid, by enema. As an antiseptic and antispasmodic methylene goes well with infusion of cinchona, with or without mineral acid. It joins equally well with soda salicylate in the treatment of acute rheumatic fever, when, with the pyrexia and swollen joints, there is much pain.

There are some other methods of employing methylene, as in combination with ammonia and with narcotics like opium and cannabis indica; but my principal object in this opusculum is to draw attention to its service, in combination with hydrogen peroxide, in the treatment of typhoid. I would not, in this stage of inquiry, venture to say that typhoid can be aborted by this treatment, but I am satisfied that under it we are approaching very near to the right plan, and that by industry in this line of inquiry we cannot fail to arrive at remarkable results. The treatment may be designated as *antiseptic oxygenation.*

ARTERIAL PULSATION AS A CAUSE OF LOCAL PARALYSIS AGITANS.

A PATIENT has recently been under my care who received about eight months ago a blow on the back of his right hand, which caused at once extreme pain in the right forefinger, followed by numbness, but without any swelling or inflammatory action. A few days after the accident the numbness passed away, except on the tip of the finger, which had not and has not quite regained its natural sensitiveness; but with the return of common sensibility there was set up a constant and marked agitation, an agitation which in a local degree resembled the paralysis agitans of the general muscular system. The agitation

remained so considerably that he was unable to use the pen for writing, and had been compelled to learn to use the second and third fingers, with the thumb, for that purpose. The agitation was most marked when the affected finger was flexed, but when it was held out straight the motion was still visible, and at all times it was out of his own control. The finger was of natural temperature; it had not atrophied, but it had a very faint blush of redness, and under slight compression it became blanched like a so-called "dead finger," but quickly reddened on reaction. Whilst I was feeling the pulse of this patient and at the same time was watching the motion of the finger, it struck me that the peculiar vibrating motion was communicated by the pulsating artery. I thereupon followed up the artery that supplied the finger by feeling for the pulsation, and by firm compression stopped the arterial supply to the finger. In an instant, as the supply was stopped, the agitation stopped, and the finger became as any of the other fingers in either hand. When the pressure was taken off the artery the agitation recommenced, and as often as the experiment was repeated the same phenomenon recurred. The phenomenon is in accord with what I have previously taught in regard to nervous vascular function. My view is that the arterial pulsations or vibrations are absorbed by the nervous expansion, and that the reason why we do not feel our own arterial pulsations is that they are so absorbed and pass in current along the nervous cord to the brain without check or muscular excitement when the course by and through the nerve is free. In the case now in hand the nerve had received an organic injury, and the nervous current was impeded. The course of vibration was therefore interrupted, and the

pulsations of the artery, from the heart stroke, were reflected on the muscles and gave rise to the involuntary muscular movements. It was as if a continued series of alternating shocks were being communicated to the muscular fibres. If in this manner a local paralysis agitans may be produced it is not difficult to apply the same reasonable explanation to the same condition of a limb or of the limbs. Place the obstruction in the cord or in the spinal nerves, and the phenomena of agitation uncontrolled by the will are fully explained. At the point of obstruction the vibration from the nervous terminals is arrested, and at the same point the current from the brain conveying the will is arrested. Thereupon the arterial pulsation is employed incessantly in sustaining the muscular action, and by that action the stored-up energy is dissipated. The diseased condition is a local epilepsy.

"DO UNTO OTHERS" IN THE TREATMENT OF DISEASE.

IF there were ever one occasion more than another in which the old Jewish precept, "Do unto others that which you would others should do unto you," should be acted on, it is when the occasion arises for the physician or surgeon to treat a fellow-being for the alleviation or cure of disease. I do not claim any merit for it, because it is simply the application to the particular of a general principle that is immortal in its nature, but I can say that during forty-two years of practice I have never, in any serious crisis calling for the exercise of my professional judgment, swerved from the practice of this principle. I have, I doubt not, in the hurry and habit of practice, acted many thousands of times, in common examples of

disease, on common principles of everyday practice, and have prescribed remedies offhand without any special study or any balancing of one thing against another, since the exigencies of practice render this course inevitable; but I can safely say that whenever life and death have been in conflict, in poor or rich, bad or good, I have always questioned myself. "Were I in this patient's position, what, with the same knowledge as I can now command—what would I do, or have done, for myself in order to secure the best means for relief or recovery?" And, having made up my mind on this point, I have acted upon it in entire independence of spirit, and with the after-satisfaction that, whatever the result, the course was the one, and the only one, that could leave the mind in the greatest ease and the greatest happiness. This rule is golden in practice in every way: it leads the practitioner to exercise special care in diagnosis and to keep himself well in advance in research; it encourages him to make research in new directions when he is at a standstill owing to defects in old and useless methods; it induces him to measure up all the pros and cons in regard to heroic or novel measures of treatment in extreme dangers, and to accept responsibilities in the performance of new treatments with a confidence that would otherwise be mere bravado. Still more, it teaches him not to accept, on mere speculation or risk, operative or other attempts at cure, which, out of sheer love of doing something for the sake of doing something, is the common root of the worst and wickedest quackeries in the world.

We are pressed sometimes even by well-meaning people to sanction out-of-the-way procedures, in desperate conditions, on the ground that if we cannot

suggest reliable aid we ought to sanction what is not reliable in order to give the patient "a last chance." We are too apt to treat requests of this nature cavalierly. We say too hastily, "*Yes*," if we want to get rid of the embarrassment, or we say too determinately, "*No*," if we are importuned with what we conceive to be the folly of the suggestion. If we wait, however, to apply the suggestion as to our own case, we take time to consider all the questions involved, and being ourselves free from personal anxiety, do better for our patients than we might do for ourselves in the advice we tender.

The principle of "Do unto others" stands us equally well in need when we are dealing with questions of prognosis in which the interests of patients are concerned. These are difficult questions, as the practitioner, in very early life, discovers. There are many practitioners who never can determine what is the best mode of answering the pertinent questions which anxious friends put to them. Some cast all their answers into an optimistic form, others into a pessimistic; some skilfully fence, and, like the illustrious Mr. Jobling, give an opinion that makes the doctor right whoever else may be wrong. But the best rule is neither to be pessimistic, optimistic, nor politic, but to act precisely as if the sick person were one's self or of one's own family, and to state facts fairly for and against recovery; in a word, to speak aloud what the mind is speaking to itself, granting of course that the person being spoken to is a reliable person and has a *bonâ fide* interest in the welfare of the sufferer.

The rule applies with just the same exactitude in the matter of consultations between practitioners, and if it were carried out in all its potency it would remove every necessity for those rigid rules of etiquette and

cumbersome formalities which so often annoy the public and perplex the profession. In past days of physic there was a fine feeling of this nature throughout all parts of medicine under the influence of which the trading spirit was held largely in abeyance. In these days the all-pervading influence of trading pursuits has led the profession, *nolens volens*, into a system of enterprise in specialities, whereby the precious intercommunion and "Do unto others as you would have others do unto you" is exchanged for a complacency, in which one practitioner stands as if he were ignorant in the presence of another, or, conversely, as if he were the possessor of all the knowledge; a false and ruinous position for physic and the afflicted world in either case.

DEATH IN THE SICK WARD.

MT is one of the disadvantages of hospital practice that, even in the best hospitals, no sufficient means has been adopted for keeping the fact of the death of one patient from the cognisance of many others. Around the bed of the dying person in the ward a screen is properly drawn, and the doctors, sisters, and nurses in attendance give their timely aid with as much gentleness and kindness as possible. They also exhibit as much cheerfulness and calmness as is possible under the circumstances, and under the influence of long professional training and the inspired knowledge "that all things are alike to all," fairly pass quite unscathed from mental suffering in the presence of death. They bend to duty, and they are in health. The surrounding sick are differently circumstanced. They are lying to get

well; they can render no assistance; they are sometimes themselves near the same fate as the dying; and to the scene that is going on so near them they are, as it were, forcibly drawn, occasionally for some hours at one time. Again, it happens that a patient dies suddenly in his or her bed with a sick person on each side. In every case the shock and depression are manifest. I never enter a ward soon after a death in it without witnessing a bad result, sometimes in those upon whom, at first sight, such an effect would be least expected.

Practically there is injury from this necessity of hospital regime, and, theoretically, there must be, in the opinion of sensitive people, a great injury. It is right at the same time to say that the injury is not so severe as many would expect.

> "To each his sufferings all are men
> Condemned alike to groan,
> The feeling for another's pain
> The unfeeling for his own."

And in the sick ward the "unfeeling" largely counterbalance the worst effects of the feeling for another's pain. Here are reasons why the sick ward is tolerable. In sickness also there is a helplessness of suffering which excludes sympathy. There is yet another relief. For one person who dies in a ward, many recover. Death is an occasional intruder; recovery is a regular visitor. And, lastly, there is a consolation in appreciating the fact that if the worst comes to the worst every care, every attempt to save, every little wish, will be attended to up to the end. These are compensating agencies, compromises with the inevitable which are of sterling value, and which soften many a painful scene.

The difficulty of isolation is insuperable as things now are. Without the hospital where would so many of the dying poor be? The hospital is not perfect absolutely; relatively it is all but perfect. In younger days I was led to think the difficulty would be met by giving to each patient a separate ward or cubicle. Unfortunately the expense attending this plan would prevent the full usefulness of the establishment. The demand "for beds" in a well-known and approved sick hospital is ever pressing; space is at all times valuable, nursing of inestimable value; and these would all be curtailed by the separate ward system. There are other objections which would not be suspected by those who do not understand hospital manners and customs. If suffering brings its selfishness, it brings also its desire for communion. I have frequently been told by the sick that they would rather be "in company" than left for hours and hours alone in their own room. Those who can read are relieved by the company of books, but readers are few in number compared with the many to whom a book is a dead companion.

On the whole, I fear we must put up with the difficulty I have named, as belonging to hospital practice, so long as such practice exists. It were well to attach to every large ward one or two single wards, into which those who are expected to die may be moved, and which could be so placed that when a death occurs in them the dead can be removed without the attention of the living. In such rooms all the special necessities for the treatment of the dying could be at hand, special temperatures could be provided, and special operations could be performed under which a small percentage of those who would otherwise die would have a chance of being restored to life. Wards of this character are

demanded in every hospital, and the rich who would wish to make a special endowment could not make a better endowment than a ward for those of their brothers or sisters who are *in articulo mortis* and have no chance of being saved except in a place where science has at hand every aid against the last enemy.

BED-CLOTHING FOR THE SICK.

IN hospital as well as in private practice great errors are made in the matter of bed-clothing for the sick, and particularly for the sick who are suffering from febrile affections. We have got rid of the heavy curtains around the bed; of the grand accumulator of dust and other uncleanliness, the tester; of the heavy vallance which converted the under-part of the bed into a close cupboard, in which all kinds of unwholesome and cumbrous articles lay concealed, including sometimes excreted matter itself; and we have banished the carpet, which, often as a hard-trodden dust-laden rag, made the floor beneath the bed persistently impure. This is all good reform, but we have still not advanced sufficiently in the reforms necessary for bed and bedding. The old feather beds, flock mattresses, heavy blankets, thick, impermeable, and dense counterpanes, still encumber many a patient, rendering ventilation of his body as impossible as in the days of our forefathers. It does not indeed seem as yet to have been accepted by physicians, still less by nurses and patients, that the body calls for ventilation; that a bedroom or ward may be the purest in a general point of view, and yet that the advantage which ought to arise may be considerably curtailed by the unwholesomeness of the bed and bedding, and by the

patient making an unwholesome atmosphere for himself in his immediate surroundings. The universal improvement that is now called for in the direction named consists in substituting porosity for density in all articles of bedding. The thick dense bed and mattress require to be replaced by the light steel elastic bed; and the clothing under and upon the patient, now so close and heavy, requires to be replaced by clothing that is porous, so that it can be permeated with pure air from without, and can at the same time permit the warm and impure air from the patient to have free exit. Under such condition of clothing there is a double current of gases going on in the clothes, which is most purifying, cooling, and refreshing; the noxious odours which so easily accumulate under dense bed-clothes have then no abiding-place; and febrile heat is dispersed instead of being retained as an addition to the evil that already exists.

The mistake now so generally made lies in the idea that the warmth which the bed calls for is best obtained by close material and close packing. The error is positive. There is nothing that retains warmth in so good and equable a manner as common air at rest. Dense materials, as Count Rumford demonstrated, cannot keep the body respirably warm. If they are non-conductors they may retain the heat, but then they retain also the cutaneous transpiration; whilst air, a splendid non-conductor, permits the freest diffusion of cutaneous emanations. Materials therefore both for the bed and for the bed-clothing ought to be porous to a free mechanical extent of porosity. The rule holds good for the clothing of the body in health; in sickness it is imperative.

CHANGES IN THE SICK-ROOM.

WE are too apt in the sick-room or sick ward to think only of the physical purity of the place and of the health of the bodies of the sick, without taking into proper account the health of the mind. This is a great mistake. Every mental act does something good or bad for the physical condition. The pulses vary with the thoughts. When we are taking a reading of the pulse with the sphygmograph or sphygmophone we find, if the patient be not looking on at the process, a certain number of pulsations; but directly the sick person sees the process, observes the needle moving, and understands that the movements are produced by the action of his or her own vessels, the pulse invariably rises, and so often remains high during all the operation that the experienced operator has to take the increased motion into account in appraising his results. No two physicians counting the pulse of a sick man make the number of beats precisely the same if the shortest time intervenes between the two takings. I once observed a difference of no fewer than twenty-two beats between my counting of a patient's pulse and that taken a minute previously by my medical brother who was in regular attendance. The patient explained the reason of the difference. "I am a little bit flurried whenever I see a strange face." These are passing changes, and may not materially alter the course of a disease; but when any cause is at work that for long periods keeps the mind excited or depressed, the mischief, small as it may seem, must be extremely great. In the sick-room or sick ward it is not usually excitement that has to be quelled, although that may occasionally be the case; it is monotony that has to be met. In the ward where

many rest, monotony also is often combined with anxiety. "Begone, dull care, I pray thee, from me," can never be expected by the afflicted amongst the afflicted. There is always something in progress, some cry, some groan, some cough, some restless movement, some expression, some sight, that adds to the personal infliction, and which, as it adds, is wont to attach itself to some fixed object looked at at the same moment. The knowledge of facts like these is suggestive. It suggests frequent and reasonable change of scene, one might almost say scenery, in the sick-chamber. Flowers are always a source of pleasure, and when fresh flowers cannot be obtained good artificial flowers, such as the rich place on their dinner-tables in days of frost and snow, are better than none, for colour alone brightens up the mind. But flowers should often be changed, both in form and in position. Pictures are good in the sick-chamber when they are bright and cheerful; but they too become very monotonous when they are to be seen for weeks at a time in one spot on which the sick eye must ever be resting. Furniture itself may be quietly moved about around the sick with advantage. The change is indicative of something done, and has hope in it of still further approach towards recovery.

STYPTIC COLLOID WITH CALOMEL OR WITH MERCURY BICHLORIDE.

IN cases of phagedænic ulceration and of syphilitic sore styptic colloid as an application goes excellently with calomel. The combination can be made in one of two ways. The calomel can be lightly dusted over the ulcerated surface in an even layer, and the styptic can

then be painted over the surface with a camel's-hair pencil; or the calomel can be well admixed with the colloid in the proportion of three grains to the fluid drachm of colloid, and a portion of the mixture can be painted over the affected part. In examples of ulcerated fauces from specific disease the application of calomel in this manner performs a double service: it acts locally and, by absorption, generally.

Mercury bichloride can, in like manner, be used in combination with styptic colloid in the proportion of one grain of the sublimate to two fluid ounces of the styptic. In the solution so formed there is sufficient alcohol to take up the sublimate, and there is no action on the tannin or collodion to cause precipitation. A few minims of this solution can be laid over an ulcerating or suppurating surface with a brush, or the solution can be applied on thin layers of cotton wool as a dressing that can be removed and renewed as required.

AN ANTI-CHOLERAIC MIXTURE.

THE most useful anti-choleraic mixture I have ever employed is one in which creasote is combined with opium, on the method first suggested by the late Mr. H. Stephens, and, quite independently, by Mr. Spinks, of Warrington. The formula I prescribe runs as follows:—

℞ *Creasoti Puri*, ℥xij; *Tinct. Camph. Co.*, ʒvi; *Spirit. Ether. Chlor.* ʒiv; *Syrup. Rhœodos*, ʒij. *Fiat Mistura. The mixture—twelve doses. One fluid drachm, or a teaspoonful to be taken every hour, or as may be directed, in half a tumbler of water.*

SIR THOMAS BROWNE, M.D., AND THE "RELIGIO MEDICI."

AFTER the publication of the last ASCLEPIAD I discovered in the life of Sir Kenelme Digby a somewhat important omission. I failed there to state that whilst Sir Kenelme was confined in Winchester House he received a letter from Edward, Earl of Dorset, recommending him to read a book which was at the time (1642) the talk of the town—namely, the *Religio Medici* of Sir Thomas Browne. The famous work had not at that time been published authoritatively, but Digby after a hasty perusal of a copy of it—for which he sent a special messenger to St. Paul's Churchyard, where the publisher, Andrew Crooke, resided—wrote offhand a criticism or review of it, completing his notice within twenty-four hours, to the apparent annoyance of its author, who pleaded that the review should not be published until the work itself was complete.

I was annoyed at first at this omission, but have appeased my mind by selecting the author of the *Religio Medici* himself for notice in the present number. The one life naturally leads up to the other, and affords grounds of contrast which it may be instructive to rest upon. Both men, Digby and Browne, are remarkable

characters, and have more than once been held up together, with the questioning of Hamlet, "Look thou on that picture and on this," as if the picture of Kenelme Digby paled before that of his gifted co-temporary; as if Digby were a pretender, Browne a philosopher; as if Digby were a casual and loose observer, Browne a philosophical investigator; as if Digby had a mere show of mental quality, Browne true genius; as if Digby were an actor inspired by popular adulation and approved only by the vulgar, and as if Browne were the man inspired by nature.

When in the year 1864 I was in Norwich, engaged in writing for the *Medical Times and Gazette* the medical history of that famous city, I was shown in the museum connected with the hospital there what purports to be the skull of Sir Thomas Browne. The skull was in an excellent state of preservation, and I then reported on it that, although it was rather above the ordinary size, it was not altogether Jovian. The forehead is very retreating, and the animal part, if the phrenologists are right, is rather too much developed for so good a man. I added that independently of the balance of power theory, a theory as useful to the disciples of Gall as to those of Machiavel, the skull conveyed to the mind the idea that its owner "was a perceptive rather than a reflective man," a view which was sustained by the readings of the skull made by the learned antiquarian Fitch, who unearthed it, with the other parts of the skeleton of Browne, from the chancel of the church of St. Peter's, Mancroft, Norwich, in the year 1847. In addition to these notes on the skull of Sir Thomas Browne, I offered a few observations on his life and life's work after a study of his *magnum opus*, the *Religio Medici*, and drew from that study and from

that of his other works the inference "that the true character of Sir Thomas Browne has yet to be written."

Since that time, twenty-eight years agone, much has been published respecting this remarkable figure in medicine. Dr. Greenhill's splendid edition of the *Religio Medici* has appeared adorned with the beauty of faithfulness and fidelity that distinguishes every page falling from the pen of our greatest living scholar and critic; and, quite recently, two new editions of the work have appeared in cheap form, with short details of the life of their author, for popular reading. The time, therefore, is in joint for the review in this place of a chapter in medical biography that has a true national history connected with it as well as a philosophic. In this review I shall treat on the works rather than the man, but, according to custom, I shall venture first to present, in simple outline, the man to my readers as he was known and appreciated in his day and generation.

BIOGRAPHICAL ABSTRACT.

It is probable that no physician has received so much attention from eminent biographers as Sir Thomas Browne. Something about him of fascination has led to his fame as a classic, and it is as a classic rather than as a physician he lives. He wrote a brief autobiography for Anthony Wood. A volume of *Minutes* relating to him was written soon after his death, at the request of his widow, by the Rev. John Whitefoot, with some additional information communicated to Bishop Kennet by his daughter, Mrs. Lyttleton. A distinct Life was published in 1712, with his posthumous works. In 1736 another Life appeared in combination with a new edition of the *Religio Medici*, and in

1756 the great Samuel Johnson wrote a most interesting memoir for the second edition of another work of Browne, entitled "*Christian Morals.*" In the "*Biographia*" an article came from the pen of Dr. Kippis. Other short and incidental memoirs followed these, but nothing considerable until Simon Wilkin, F.L.S., edited the complete works of the author in 1836, in what is known as the Pickering edition, published by William Pickering, one of the most enterprising and excellent publishers that ever adorned the trade. Quite recently Dr. Lloyd Roberts, of Manchester, has written for the "Stott series" a faithful and condensed Life as a prelude to the principal works of Browne. With these a most useful chronological table by Dr. Greenhill in his splendid edition of the *Religio Medici* and other works of the author—published by Macmillan in 1881—should be read.

In his most famous book, the *Religio Medici*, Sir Thomas Browne speaks of his life up to thirty years "as a miracle, to relate which were not a history, but a piece of poetry." Dr. Lloyd Roberts in the opening page of his memoir quotes this passage with wonder, and I am not surprised at it. I agree with Roberts that a less eventful life was scarcely ever realised. The most stirring events were taking place during the latter half of the memorable thirty years included, but in those events Browne took no part whatever. Then, and all through his career, he seemed dead to the world outside of him and alive only to his own pursuits and reflections. A pure egoism ruled his life; he never lived out of himself; and it was this introspection, this reading of himself by himself, that made his life, to him, the miracle he deemed it to be. I doubt whether there is to be found anywhere in history such a truly self-contained man. In section 8, Part II., of the *Religio*

he is more communicative about himself than ever. He inherits, he informs us, millions of vices from Adam, but he has escaped one, *pride*. Besides the jargon and patois of several provinces, he understands, he says, no less than six languages, yet he has no more conceit of himself than his fathers had before the confusion of Babel. He has not only seen several countries, beheld the nature of their climes, the chorography of their provinces, and topography of their cities, but he understands their several laws, customs, and policies; yet cannot, he declares, "all this persuade the dulness of his spirit unto such an opinion of himself, as he beholds in nimbler and conceited heads that never looked a degree beyond their own nests." He knows the names and somewhat more of all the constellations in his horizon; yet he has seen "a prating mariner, that could only name the pointers and the North Star, out-talk him and conceit himself a whole sphere above him." These are brave words for a youth of thirty, and in modified tone they ring up to the end of his literary career.

Thomas Browne was born in the parish of St. Michael's, Cheapside, London, on October 19th, 1605 (Gunpowder Plot year). His father, also Thomas, was from Upton, in Cheshire, of a good family there, but settled in London as a merchant, and evidently a successful one, for he died early whilst his son was a child, leaving £9,000, a third of which came to his wife, and the residue to young Thomas, a brother, and two sisters. The widow, a daughter of Mr. Paul Garraway, of Lewes, in Sussex, did not remain long unmarried after the death of her husband, but became united to a gentleman named Sir Thomas Dutton, who filled some post of honour in the government of Ireland, and soon afterwards left, with his wife, for Ireland. The boy Thomas, his brother and

sisters, remained under the care of a guardian, who, it is said, was rapacious, and treated the fortune of his wards as if it had been his own. However this may be, Thomas Browne was admitted to the school at Winchester on August 20th, 1616, and proceeding to Oxford in 1623, was enrolled as a gentleman commoner at Broadgate Hall, afterwards Pembroke College. He matriculated at Broadgate, took his B.A. on June 30th, 1626, and his M.A. on June 11th, 1629. He now turned his attention to physic, and practised for a short time in Oxfordshire, but left his work there in order to join his stepfather, Sir Thomas Dutton, in Ireland. He remained for a very short time in the Emerald Isle, having little to detain him in that already down-trodden and unhappy country, but went off, as gentlemen of his time usually did, to make the grand tour and become italianised. He first visited France, and studied for a time in the great school of medicine at Montpellier, a school to which his cotemporary Kenelme Digby, as we have seen, also bent his student's steps. From Montpellier, after a short sojourn in drinking Rochelle, he passed to far-famed Padua, and returning home by Leyden, graduated M.D. there in 1633. He reached England in 1634, and probably in that year moved to Shipden Hall, near Halifax, where he commenced again to try his fortune at medical practice. He was not very successful in this place as a practitioner, but he scarcely stayed long enough to try his full measure of success.

At the same time there was this advantage in Shipden Hall to the young M.D. from Leyden: he obtained full leisure to think out his learning and observation and to compose the little book that has proved the basis of his fame. He wrote here in great part, if not alto-

gether, and in crude part certainly, the *Religio Medici.* His brain was young, his observations fresh, his confidence strong in himself and in no other man. An older mind could have performed no such feat as the production of this work. Greenhill dates its birth in 1635 with a note of interrogation. The note is wanted, but the date is not very far wrong. Browne must soon afterwards have come to London, perhaps to get the book into the hands of the printer, for printing at Shipden Hall would not be probable.

During his student life at Oxford Browne had been fortunate in having Dr. Lushington for a tutor. Lushington moved from the university to become rector of Burnham Westgate, near to Norwich, and under his persuasion, backed by that of Sir Nicholas Bacon, of Gillingham, and other influential Norfolk families, the young physician moved to the "City of Orchards," as Norwich has been designated, and settled there for life, adding a living honour to the ancient and remarkable place. In the first years of his life there he still held back the open publication of his great work, but it was known and talked about nevertheless, a passage in it relating to marriage giving rise to much amusement when in 1641 he married Miss Dorothy Mitcham, fourth daughter of Edward Mitcham, of Burlingham St. Peter's, a handsome lady of some family influence and of such maternal facilities that she bore twelve children to a husband who had declared he "could be content that we could procreate like trees, without conjunction, or that there were any way to perpetuate the world without this trivial and vulgar way of coition, the foolishest act a wise man commits in all his life, nor anything that will more deject his cool imagination when he shall have considered

what an odd and unworthy piece of folly he hath committed."

In the midst of the most momentous period of the history of this country, during the great fights between King Charles I. and the people, represented by their Parliament; during the time when the King was beheaded; during the period when Cromwell held the reins of power and made England the mainspring of the world; during the sparkle of the Restoration and the sudden transformation of Puritan sanity into libertine madness—during all these mighty revolutions Dr. Thomas Browne, a Royalist, who had subscribed money for the King, pursued the even tenor of his way as physician and man of letters at Norwich, the city of orchards. His medical practice was considerable, but he does not appear to have added any important detail to the practice of physic. His literary practice, if so it may be termed, continued, as we shall see, to grow and prosper. He had not been long settled at Norwich—1636—before the university of Oxford conferred on him the degree of Doctor of Physic. He was incorporated Doctor on July 16th, 1637, and thence onward held the first place as physician in the city of his adoption. His practice was, without doubt, lucrative; but his family was large, and he lived at sufficient expense to prevent the amassment of a large fortune.*

* Of his twelve children eight died in their infancy. His eldest son, Edward, rose to fame in the medical world, was elected F.R.S., and was President of the Royal College of Physicians. A second son, Thomas, whom he sent, unattended, at the age of fourteen, to France, entered the royal navy, showed great parts as a writer and observer of natural things, rose to the rank of lieutenant, and died probably in the year 1667, but where and under what circumstances we have no record. His eldest daughter, Susanna, married Arthur Moore, Esq., M.P. for Grantham, but died soon afterwards. His daughter Mary

He dressed plainly, but neatly, and always warmly, retaining the old-fashioned cloak and boots. He was simple and orderly in his mode of life, but his manner is variously described by different observers. Some relate that he was very reserved, others that he was open, free, and even loquacious. His friend Whitefoot, who is quoted by Johnson, Wilkin, and other biographers, and who boasted a knowledge of him for two-thirds of his life, is rapturous in his praises; but when we come to an examination of Whitefoot's description we find it is taken, considerably, from Browne's account of himself in the *Religio Medici*. Johnson, with his usual shrewdness, detected this fact, and when he copied the commendation that the famous man—who in his hands too became a hero—was one destitute of pride or conceit, winced at that statement and admitted with a less favourable critic, Dr. Watts, "that a perusal of the *Religio Medici* will not much contribute to produce a belief of the author's exemption from the father sin, pride being a vice which pride itself inclines every man to find in others and to overlook in himself." The admission is too true. Browne was essentially a man who thought well of himself and first of himself; he knew himself, one of his admirers admiringly affirms. It is quite true. He did know himself, and that is the secret why his works were born and why they were not still-born. If poor old Alex-

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died in her twenty-fourth year, in 1676. Another daughter, Elizabeth, married Captain George Lyttleton, and being left a widow, retired to Guernsey; and one more daughter, Frances, remained single after her father's decease. Of forty children and grandchildren that sprang from him and his wife not one of the male line remained after thirty years, and only one family of the female line has come down into our day—namely, the Erskine family, who descended through the eldest daughter of Anne Browne, Frances Fairfax.

ander Ross, one of his critics, sometimes called a divine, had known himself as well, he would have done better as a critic and been more successful in his opposition.

For my part, after a good course of delightful reading on the character of this brother in physic, I like best the account of him by Evelyn, who visited him in Norwich, and who formed a favourable opinion of him. "His whole house is a garden," Evelyn writes, "a paradise and cabinet of rarities, and that of the best collections, especially medals, books, plants, and natural things, amongst others a collection of the eggs of all the fowl and birds he could procure, that country, the promontory of Norfolk, being frequented by several kinds which never go further into the land. He led me to see all the remarkable places of this ancient city, being one of the largest, and certainly, after London, one of the noblest, of England." The account of this visit of Evelyn leaves a far better impression than the laudation of Whitefoot, and here for a time we will rest from personal narrative of the physician to study for an interval the scholar and classic and taste the food of his works.

"RELIGIO MEDICI."

The earliest work of Sir Thomas Browne was the *Religio Medici.* This also, taking it altogether, was his greatest work, although De Quincey and other writers have opined that in his work on Urn Burial there are the finest passages. Let the reader recall, before he goes further, the fact that the author of this famous little essay —for it is made up of less than thirty-four thousand words—had not seen thirty summers when he wrote it, and he may then perchance pardon any faults of expression he may find in it; perchance, too, he may

marvel how so young a writer could have composed a book that created in its day so great a controversy, and since its day has retained so firm a hold on the minds of men. Had it been an allegory like the *Pilgrim's Progress*, or a narrative like *Robinson Crusoe*, or a satire like *Don Quixote*, there need have been no cause for marvel, but this essay! It is a marvel.

Upon what does its success depend?

Several causes have contributed to its success. To begin, the name of the work was singularly suggestive and taking. The Churches of all sorts had long looked askance on the science and art of physic separated from the Churches. It had been promulgated that the medical professors had no religion, and the words, "Three physicians, two atheists," had become a common saying. This book claimed a religion for physic. It was an exposition of the faith of a physician. Let us see at last what that faith is. Is it an orthodox faith? Does it attach itself to any particular sect or creed? Is it a practical faith, likely to bear good works? Here was food for curiosity.

In the second place, the style of the book was novel. This is a fact which Johnson detected and lucidly exposed. The stability which our language had obtained in the age of Elizabeth had commenced to waver in the age when Browne took up the pen. Milton, Johnson argues very truly, had introduced the Latin idiom; Browne poured in a multitude of exotic words, many useful and significant, which rejected would have to have been supplied by circumlocution. His style indeed was a tissue of many languages, "a mixture of heterogeneous words brought together from different regions, with terms originally appropriated to one art and drawn by violence into the service of another."

He thus inaugurated a philosophical diction. "He had, moreover, uncommon sentiments, "and was not content to express in many words that idea for which any language could supply a single term." This novelty of style and expression, so clearly defined by Johnson, had its charm; it had its obscurity also: but that signified not so long as it was strange, forcible, and in the end alluring. It was rugged, but it was in its way poetic; and, as we have seen in one striking example in our own time, the difficulties of understanding the style rather added to than took away from its acceptability. Doubtless many read the book and liked it when they did not in the least comprehend it or desire to comprehend it.

In the third place, the very personality of the book had its influence. There were bits of autobiography in all parts, and they were sure to be popular, though self-panegyric were as clear as the day. The author tells us what he was, how he felt, how he thought, and what he did. We do not swallow all he says, and we agree with Sir Kenelme Digby that he informs us too much about himself. Never mind; it is interesting. He depicts himself as shorn of conceit and pride; we know that the very statement proclaims the opposite: he affirms that he could sit and have a limb removed without giving vent to expressions of pain; we doubt, but are not inclined to dispute, the assertion: he protests that no man living has less fear of death than he; we are not quite sure of the statement, and yet we are satisfied with it: he praises up his great knowledge of languages, of sciences, of arts; we discover that, in spite of this praise, he does not understand or does not believe in the motion of the earth round the sun: he still believes in the mysteries of witchcraft, alchemy,

and astrology; yet we permit his assertion; it is so boldly and uncompromisingly declared. In fact, this youth of thirty makes men believe in him whether they willed it or not; and when a great cotemporary taught,—

> "A man convinced against his will
> Is of the same opinion still,"

he ought to have appended a footnote: "Readers of the *Religio Medici* always excepted."

A fourth influence which caused the work to run its bright career was its poetry. It is a prose poem from its beginning to its end; but at points it glides into actual poetry, crisp, musical, sweet.

> "Search where thou wilt, and let thy reason go,
> To ransom truth, even to the abyss below;
> Rally the scattered causes; and that line
> Which nature twists be ready to untwine.
> It is thy Maker's will, for unto none
> But unto reason can He e'er be known."

This is the opening of one short poem, remarkable for the argument as well as the style. It does not continue in the same strength, "its combinations are uncouth," and, like Gray's "Elegy," it ends feebly; but, despite faults, it is attractive. In climbing the mountain of thought it is like a tree by the stem of which one can hold, and under which one can for a moment rest. Still better than the poem noticed above is the "Evening Hymn" of Browne. He is treating of sleep, and tells us sleep is so like death, "he dares not trust it without his prayers, and in half adieu unto the world takes his farewell in a colloquy with God." I have often compared this evening hymn with the popular one of Bishop Ken, which so many of us learned and

sang in childhood, and, for my part, I like this the best. How gentle and pure the opening lines :—

> "The night is come, like to the day;
> Depart not Thou, great God, away:
> Let not my sins, black as the night,
> Eclipse the lustre of Thy light;
> Keep still in my horizon, for to me
> The sun makes not the day, but Thee."

How simply grand also the definition and invocation of sleep :—

> "Sleep is a death; oh, make me try
> By sleeping what it is to die,
> And as gently lay my head
> On my grave as on my bed:
> Howe'er I rest, great God, let me
> Awake again at last with Thee;
> And thus secured, behold I lie
> Securely or to awake or die."

This was the "dormative he took to bedward." It needed no other laudanum to make him sleep; after that he closed his eyes in security, content "to take his leave of the sun and sleep unto the Resurrection."

Most readers will see the analogies between this hymn and that of Bishop Ken.

> "Teach me to live that I may dread
> The grave as little as my bed;
> Teach me to die that so I may
> With joy behold the judgment-day."

There, in rounder and more rolling rhythm, is similar thought so similarly rendered, it looks as if Ken had merely paraphrased Browne; but, the probability is, as Greenhill acutely observes, that both poets, scholars

of Winchester, found in the old songs and hymns in use amongst the scholars, the originals on which, according to their genius, both Browne and Ken tuned their lyre and left their music on the ear of the world.

I have assigned now several, and perhaps sufficient, reasons why the *Religio Medici* took and retained its hold on men—on men, not women, for until quite lately it has been too exclusively a man's book. But there is another, and much more important, reason for its success: it is a book that leaves behind it a mystery, and a mystery is of all things that thing which the mind of man most loves. Mystery belongs to us as a divine mantle in which we are enshrouded, a living winding-sheet. The sun, moon, stars, earth, air, animals, plants, our own bodies and minds, all are mysteries. We are mystery, and what we are we dream of most in our waking dreams. Even when we believe we know we are still mysterious. As Madame de Stael aptly expressed it, "What we know is as inexplicable as the unknown." Ah, wise Madame, so it is! And this book, ever a mystery, remains a mystery.

What is the religion of a physician, after all? That was the question the world asked and asks of the *Religio Medici*. In the first days of the book, a Roman Catholic argued, "This is the book of a believer in the true Church"; a Quaker speculated, "This book must be by one of my community of thought"; the illustrious Bayle, after pithing it mercilessly, says, "The *Religion of the Physician*, which, according to some, might be intituled *The Physician of Religion*, is a work of such a cast that many have imagined the author of it a little remote from the kingdom of heaven"; and Guy Patin, one of the learned in physic, acknowledging that there are many fine things in the book and a

pleasing melancholy in the thoughts of it, says of the author of it that he has, like many others, a religion to seek, and perhaps will find none at last.

Putting aside the vehemence of Alexander Ross—who, by the way, I rather admire as a faithful and candid writer, very little read even by the religious community—we see that to staid and learned men the *Religio Medici* was a stumbling-block. I am quite sure that Samuel Johnson, though in the end he made up his mind to take the volume under his wing, had fearful flutterings of the heart and twinging doubts before he put it under his wing. And so to this day it remains a doubt. It may be worth while, having given some reasons why the book took hold of the world, to glance at it for ourselves and read it face to face with the best desire to comprehend it in its depth as well as its simplicity.

When, in 1864, I had in my hands the skull of the author of the *Religio Medici*, I determined to make a careful study of the book, and in a few weeks had, I believe, the contents of it as tightly in my brain as ever it was in the skull in which it was generated. The book fascinated me; and although I almost learned it by heart, it was a close puzzle. Under first impressions I wrote down the idea, for reasons the book itself supplied, that it was satire, having then no knowledge that other readers, some of whom were masters in criticism, had come to the same conclusion. Since 1864 this book has been a bedside companion, one of the chosen companions I keep by my couch and read in early morning once yearly, in order to remain well up in them. Longer reflection and more extended communion with this book have rather modified my earlier views, although I am still forced to recognise a vein of satire running

through it. I perceive with Bayle it is not so truly the religion of the physician as it is the physician of religion. I see, too, that, as a religion of the physician, it is not one of the physician in his universal acceptation, as most suppose, but the religion of the one physician who composed it, Dr. Thomas Browne. It could not be the religion of all the faculty, but it could be that of one of the faculty, and there it must remain, fanciful, satirical, contradictory, yet as earnest a work as any young man could under the circumstances have written.

To understand the argument, if that can be styled an argument which consists of disjointed propositions clothed usually in roughly decorated language, requiring to be mastered step by step before the end is gained and the intention seen, it is essential to recall the environments in which it was conceived. The majesty of the Catholic Church of Rome had then departed from the minds of men of the class to which Browne belonged, except in such instances as that of Kenelme Digby, where heredity carried the majesty almost intact. At the same time the religion of the Puritan and Presbyterian, the Baptist, the Anabaptist, and the other sectaries was to this author harsh, low, vulgar. He therefore found refuge in the reformed Church of England, with sufficient ceremonial in the services of that Church to give poetry to them and a mixed system of learning as well as worship. That is not all. He was smitten with science, not deeply, not by experimental personal labour like Galileo, nor by psychological labour like Spinoza, but by a kind of secret sympathy which he dared not fully confess even to himself. In the dilemma he strove to bring up reason to the bar of faith, and when in the attempt he found

reason compatible with faith he was satisfied; whilst, when he found what he called reason in opposition to faith, he gave reason its *congé* and let faith stand alone.

This is the key of the mystery, the real *Religio Medici* of Thomas Browne the physician. When rebellious reason puzzled him, he preferred to lose himself in a mystery up to an "O altitudo"; and when in the name of reason the devil ensnared him, he was content to run for protection into the paradox of Tertullian, "This is certain, because it is impossible" —"Certum est, quia impossibile est." He believed, under the lure of faith, there was a tree in the garden of Eden whose fruit our unhappy parents tasted, "though in the same chapter of the book of Genesis in which God forbids it 'tis positively said the plants of the field were not yet grown, for God had not caused it to rain on the earth." He believed in fact, so at least he attested, all to be true which, indeed, his reason would persuade him to be false, and thought it no vulgar part of faith to believe a thing not only above, but "contrary to, reason and against the arguments of our proper senses." "The following words of the Gospel ought, therefore," says philosopher Bayle, "to be applied to him: 'I have not found so great a faith, no not in Israel.'" Because of the witch of Endor and of other references to witches in the Bible, he believed in witchcraft; and later in his life positively assisted at a trial at Bury St. Edmunds, over which Sir Matthew Hale presided as judge, in sending two poor miserable wretches to death for giving fits to one or more who died from epilepsy. "The fits," he swore, "were natural, but heightened by the devil's co-operating influence with the malice of the witches, at whose instance he did the villainies." Uncompromising admirers of Browne regret this evi-

dence on witchcraft; I regret the foul result of it as much as any one, but I feel, notwithstanding, that it clears the author of *Religio Medici* of the fouler offence of writing in satire what he did not believe and of being as bad as the Machiavel whom he scathingly condemns.

It might be expected that these arguments of faith *versus* reason would have secured Dr. Browne from all charges of Atheism, Deism, or other schismatic or heretical opinion. They did not protect him. The Pope placed the book in the Index Expurgatorius, although the author went so far as to commend or at least favour the system of praying for the dead and showed sympathy with Roman ceremonial. The sectaries opposed it as a dangerous book all round.

Where lay the danger? The danger lay in two directions. In one direction, a certain dogma at that time intensely popular amongst the Presbyterian community was pushed to its logical end with an apparent force and subtlety that raised an issue beyond ordinary exposition or argument. Whether Browne were an Augustinian or a Pelagian I know not; but either really or satirically he professed an absolute faith in election and predestination which "Holy Willie" himself could not have beaten. "The world," he says, "was before the Creation, and at an end before it had a beginning; and thus was I dead before I was born: though my grave be in England, my dying place was paradise; and Eve miscarried of me before she conceived of Cain." Was this the "*argumentum ad fidem vel absurdum*"? Who could decide? In another passage he speaks of himself as overjoyed that he had not seen either Christ or His Apostles, and that he did not live in the age of the miracles, for his faith had then been involuntary, and he would have had no share

in the following blessing: "Blessed are they that have not seen, and yet have believed."

That burning tongues on religious subjects should throw doubts on the sincerity of an author who appeared to protest too much was not in the least surprising; and, without doubt, the opposition was aggravated by the liberal introduction of scientific assumption which to the mind of the world generally was new, and to part of that mind terrible. For example, he seized the text, "All flesh is grass," and on that preached not only its metaphorical, but its literal, truth. All creatures, he declared, are but herbs of the field digested into flesh in them, or more remotely carnified in ourselves. "Nay, further," he continued, "we are what we all abhor—*anthropophagi* and cannibals, devourers, not only of men, but of ourselves, and that not in an allegory, but in a positive truth, for all this mass of flesh which we behold came in at our mouths; this frame we look upon hath been upon our trenchers; in brief, we have devoured ourselves." Who need be surprised that a man writing such wild heresy in the era of the Commonwealth should be doubted? Why, the dread of it has not passed away now, for when in one of my own lectures I explained that all flesh is grass, that the lower animals of the herbivorous class are our laboratories, and that the pure carnivora could not live without their interposition; and when, going a little further, I suggested that we ought to learn in our chemical laboratories how to transmute the grass of the field and other vegetable substances into foods as condensed and nutritive as animal foods, my teaching was received as heretical, at best a dream of science, and something about which the least said the soonest mended.

The author of the *Religio Medici* himself was not

blind to the objections that would be made to his work. He let it appear at first half authoritatively in print, and in the hands of Croke, the publisher, who sold it. The process was a pioneering one. The book got into the possession of Kenelme Digby, who animadverted upon it too hastily, truly, but with signal ability. In its nascent state it floated several years before its parent owned its origin; and when, at last, he put his name to the volume and "inclused" it with his life, he confessed that there were many things in it "to be taken in a soft and flexible sense, and not to be called under the rigid test of reason." Also he "submitted all that is contained in the volume unto maturer discernments, and would no further father them than the best and learned judgments shall authorise them."

I have traced now, I trust fairly and honestly, the mental construction of the *Religio Medici*. In epitome it is made up of the thoughts of a young medical poet: thoughts written in solitude; thoughts of a mind in which science had struck a spark that would not go out, and yet could not burst into flame; thoughts of a mind in which, as in the mind of Shakespeare, the glorious realm of the supernatural, the angel, the spirit, the fairy, the devil, the imp, the witch, the wizard, the physical heaven, the physical hell, still held place and power; thoughts of a man in whom religious faith controlled reason; a man who, distinguishing plainly his difficulty, met it by such extreme acquiescence in faith, he seemed to scoff when he really prayed, and whose book was, and remains, a mystery because of the very simplicity and sincerity of its heart. To me once a satire, it breathes now a confession of a struggling scholar, of a true child of science, a poet striving to read from nature, understanding some parts, but closing the

page as undecipherable and too fearful to be pursued whenever doubt proved traitor.

Of all books in the English language the *Religio Medici* has, I believe, had bestowed upon it the most fostering care. It was famous from its first. A Mr. Merryweather turned it quickly into what Johnson terms "not inelegant Latin," and from Merryweather's edition it was translated into Italian, German, Dutch, and French. Of English editions, with in some cases other works of the author and biography—one of which made the literary reputation of an Oxford student named Chapman in 1831—Greenhill reckons up thirty, to which must be added his own precious edition and two others published at popular prices this year. The care Greenhill has bestowed on his edition is wonderful. His table of contents supplies an analysis of the "*Religio*" and of "*Christian Morals*," giving in short sentences the texts of the author in a style most comprehensive and instructive. In fact, it is a model of editorial skill, in which industry contends with accuracy, and both with erudition.

"INQUIRIES INTO VULGAR AND COMMON ERRORS."

"Browne," says Johnson, "having entered the world as an author and experienced the delights of praise and molestations of censure, probably found his dread of the public eye diminished; and therefore was not long before he trusted his name to the critics a second time, for in 1646 he printed *Inquiries into Vulgar and Common Errors*." This book, a much more extended labour than *Religio Medici*, is free of all fancy and full of detail. It was published in 1646, and soon appeared in second and third editions under the hand of its author.

I am writing with a fine copy of the third edition, once the property of Henry Kirke White, before me. The title-page is printed "*Pseudodoxia Epidemica; or, Inquiries into very many Received Tenets and Commonly Preserved Truths, corrected and enlarged by the Author, with Marginal Observations and a Table Alphabetical at the End.* Date 1658. Publisher, Ekins, at the Gun in Paul's Churchyard." It is a volume of 326 closely printed quarto pages, and is filled with curious notices and corrections and, if the truth must be told, errors of its author. It is strange to read now "that Satan deceives man by trying to make man believe that apparitions and such as confirm his existence are either deceptions of sight or melancholy depravements of phancy; and that he (Satan) endeavours to propagate the unbelief of witches, whose concession infers his co-existency." It is marvellous to find here an argument that the earth is a fixed body, and that the sun moves round it; still more astounding is it to discover a physician classing amongst vulgar errors the fact that the heart of a man lies on the left side, and that, strictly taken, the heart is seated in the middle of the chest. These and many other similar statements are indeed peculiar, and make one prefer to call the book by its original title alone: *An Inquiry into very many Received Tenets and Commonly Preserved Truths*, rather than into vulgar errors, as he (the author) does later on. But, with all these lapses of his time, Browne has some acute observations on various subjects, savouring, however, rather of criticism than of refutation. Thus Aristotle, having noted that the horse has no gall-bladder, directs attention to the fact, and draws the inference that whole-hoofed animals have no gall. This inference he, Browne, sets down as a vulgar error,

because, "although the horse has no bag or vesicle which long containeth this humour, the gall, yet there is a manifest receptacle and passage of gall into the intestines, which being not so long detained as it is in other animals, procures the frequent excretion in this animal." The criticism is sound and ingenuous, but it is rather the correction of a scientific oversight of a great master than of a vulgar error. To be brief, though there is some science in the book, it is crude, and often itself superstitious, with no terms in it, unless we admit one, "hip gout" for sciatica, that need be prized. Thomas Willis would have scorned such a treatise; Harvey would have disowned it; and it is almost certain that if it had preceded the *Religio Medici*, both works would have passed into obscurity of obscurity.

"HYDRIOTAPHIA—URN BURIAL."

It is refreshing to turn from *Vulgar Errors* to the classical and beautiful treatise on Urn Burial, a discourse on some sepulchral urns found in Norfolk, published by Browne in 1658, with an epistle dedicatory to his worthy and honoured friend Thomas Le Gros, of Crostwick, Esquire. Some urns had been found in a field at Old Walsingham, between forty and fifty of them, deposited in a dry and sandy soil, not a yard deep nor far from one another. These formed the text of a discourse in which the author shines at his best. He returns here from imperfect detail into his own plaisance of poetry and history. The opening stanzas in the epistle are truly magnificent, so musical, so far away from the earth, and yet so embedded in it:—

"But who knows the fate of his bones, or how often he is to be buried? Who hath the oracle of his ashes,

or whither they are to be scattered? The reliques of many lie, like the ruins of Pompey's, in all parts of the earth; and when they arrive at your hands, these may seem to have wandered far, who, in a direct and meridian travel, have but a few miles of known earth between yourself and the pole."

In studies such as those before him he saw the sphere of the learned physician. In his previous work he had commended the out-of-the-professional-way labours of William Gilbert; now he commends his own work of the same extra day character:—

"Beside, to preserve the living and make the dead live, to keep men out of their urns and discourse of human fragments in them, is not impertinent unto our profession, whose study is life and death, who daily behold samples of mortality, and of all men least need artificial mementoes or coffins by our bedside to mind us of our graves."

There are five chapters on Urn Burial. Each is rich in history, philosophy, fancy, with touches here and there of science and once of scientific discovery, for he tells us that "in an hydropical body, ten years buried in the churchyard, he met with a fat concretion where the nitre of the earth and the salt and lixivious liquor of the body had coagulated large lumps of fat into the consistence of the hardest Castile soap, whereof part remaineth with us." This was the actual discovery of the substance since named *adipocere*. He favours cremation on several grounds, among others one that "to be knaved out of our graves, to have our skulls made drinking bowls and our bones turned into pipes to delight and sport our enemies, are tragical abominations escaped in burning burials."

The choicest pieces on urn burial are in snatches,

proverbs, or shall I not rather call them texts? Here are a few :—

"We slightly remember our felicities, and the smartest strokes of affliction leave but short smart upon us."

"Sense endureth no extremities, and sorrows destroy us or themselves."

"Afflictions induce callosities; miseries are slippery, or fall like snow upon us, which notwithstanding is no unhappy stupidity."

"To be ignorant of evils to come and forgetful of evils past is a merciful provision of nature."

"Life is a pure flame, and we live by an invisible sun within us."

"Diuturnity is a dream and folly of expectation."

"Our days become considerable, like petty sums, by minute accumulations, where numerous fractions make up but small round numbers."

"There is nothing strictly immortal but immortality."

"CHRISTIAN MORALS."

After the death of Browne, his daughter, Mrs. Elizabeth Lyttleton, dedicated to David, Earl of Buchan, a posthumous treatise of her father entitled "*Christian Morals.*" The Archdeacon of Norwich, John Jeffrey, D.D., prepared it for press; it was printed at the University Press, Cambridge, for Cornelius Cromfield, printer to the university, and was published by Knapton, of the Crown in St. Paul's Churchyard, in 1716. Several editions of this work have been since published. Greenhill reckons up thirteen, to which must be added his own and two at least of later date. I do not wonder that *Christian Morals* should be a favourite tract, for it is of fine quality, free of all cant, full of sound philosophy, heathen, if the word be not a misnomer, as well as

Christian. The keynote of it is that "the created world is but a small parenthesis of eternity," and the true moral of life to live under this impression. Here again text follows upon text, line upon line, precept upon precept. There are also touches of original quality which indicate the close observer. What would Gall and Spurtzheim have said to the following passage as a forecast of phrenology?—

"Since the brow speaks often true, since eyes and noses have tongues, and the countenance proclaims the heart and inclinations, let observation so far instruct thee in physiognomical lines, as to be some rule for thy destination and guide for thy affection unto such as look most like men. Mankind, methinks, is comprehended in a few faces, if we exclude all visages which any way participate of sympathies and schemes of look common unto other animals."

This is but one taste of many good things which every true student of medicine who would "swim smoothly in the stream of his nature and love but one man" should learn. I would I could afford to extend the pages of ASCLEPIAD more widely for the wisdom of this book of goodness undefiled, but I have expanded too much already and must summon the conclusion.

MISCELLANEOUS WORKS AND LETTERS.

There are a few other papers of our author that call for brief notice at least. *A Letter to a Friend upon the Occasion of the Death of his Intimate Friend*, published for Charles Brome, of St. Paul's Churchyard, in 1690, is a touching, though rather discursive, narrative with some medical teachings in it that deserve remembrance. Browne attended this sick man, and was struck by an "odd mental symptom not mentioned by Hippocrates":

that he (the sick man) "lost his own face and looked like some of his near relatives, for as from our beginning we run through variety of looks before we come to consistent and settled faces, so before our end, by sick and languishing alterations, we put on new visages, and in our retreat to earth may fall upon such looks, which, from community of seminal origins, were before latent in us." An excellent and true lesson in diagnosis.

As an addition to the little work on Urn Burial, he wrote *The Garden of Cyrus; or, The Quincunxial Lozenge, or Network Plantation of the Ancients, artificially, naturally, mystically Considered*, a fanciful sketch, deducing the practice of horticulture from the earliest antiquity to the time of Cyrus, who first planted a quincunx, and dealing with the importance of the number five.

The letter to a friend written in 1672 is considered to be the last work, in set form, of this author, and here all special notices may end, with one exception. There is extant a letter which he wrote in 1647 to Dr. Henry Power, of New Hall, Yorkshire, on the *Education of the Practitioner of Medicine*, a letter which, as indicating the most advanced study of physic in the middle of the seventeenth century, is worthy of note. Men, he tells Power, can never become physicians from books, but "Galen and Hippocrates must be read as fathers and fountains of the faculty. Lay your foundation in anatomy; read also Vesalius, Spigelius, and Bartholinus; be sure to make yourself master of Dr. Harvey's piece *De Circul. Sang*, which discovery is preferable to that of Columbus; study plants, animals, and minerals, the *materia medicamentomorum*; be not a stranger to the useful parts of chemistry; and so by degrees march on. Having learned anatomy, read two or three times over Sennertus's *Institutions*, after which you will seldom

meet with any point in physic in which you will not be able to speak like a man. See how institutes are applicable to practice, but in reading satisfy yourself not so much with the remedies set down as with the true understanding of the nature of the disease, its causes, and proper indications for cure. If at one view you would see who hath written and upon what diseases, by way of counsel and observation, look upon Moronus's *Directorium Medico-practicum.* You may look upon all, but dwell upon few. The words of art you may learn from Gorreus's *Definitiones Medicæ.*"

SIR THOMAS BROWNE.

Returning to a final interview with the illustrious man of genius whose works we have surveyed, I may, before bending my last adieu, glance at the final stage of his earthly career. In December, 1665, he was admitted as Honorary Fellow of the Royal College of Physicians by special diploma. On September 28th, 1671, King Charles II., with what Wilkin correctly calls "his profligate court," visited Norwich from Newmarket. In this visit to the ancient city the King wished, as Johnson says, to reward virtue "with what cost him nothing," and therefore offered knighthood to the Mayor. The Mayor, for some reason little known to mayors generally, prayed off the honour, which thereupon was thrust on the great doctor of the city, henceforth Sir Thomas Browne. From this date, still widely known and beloved, and ever with his hands full of work, he wore his well-earned fame, became wealthy, made his will on December 2nd, 1679, and on the day of his nativity, October 19th, 1682, succumbed to an attack of colic, and, as he would have declared, "resigned his soul to the God who gave it."

ORIGINAL RESEARCH, EXPERIMENTAL AND INDUCTIVE.

"Dire n'est rien ; faire est tout."—RENAN.

RESEARCHES ON VITAL COHESION.

IN the paper on Vital Hydration in last ASCLEPIAD it was shown that crystalloidal substances, like urea, are essential for the vital excretion of fluids from the living body. It was shown that these saline substances seize the water from the tissues, fix it, and carry it away through the proper channels necessary for its escape. Universal dropsy is thus prevented.

There is another act which has to be considered in this process of dehydration, and that is cohesion. We take in water with our drink: it is one of the most necessary of vital acts; we produce water in the chemical changes of combustion within our bodies: it is a necessary part of the combustion process. But the water supplied from both these sources, imbibition and production, what must become of it? Where must it accumulate, and in what manner, before it is fixed by saline matter and disposed of by excretion? It must, to use a homely expression, be sponged up.

It is sponged up by those structures which we call jelly-like substances shortly, after Graham, *colloidal*. The colloidal structures in the animal body are all

the living parts. The blood during life is not a mere mixture of water and of substances suspended in the water, but a true colloidal liquid in the active state. In it, each performing active functions, are the colloid fibrine, the albumen, and a third colloid, which I find always separates from blood, like a pellicle of caseine, when blood is evaporated. In the same fluid float the semi-colloid corpuscles, mixtures of crystalloid and colloidal substance.

Out of the blood there is spread, through the body, an infinite series of colloidal structures: the great sheet of cellular membrane or tissue; the membranes enveloping all the organic parts; the lenses; the cartilages; the muscles; the tendons; the organic part of the skeleton; the skin; the mucous membrane.

In fluids yielded by glands, to which the name of secretion may be applied, there is evidence of a colloid. In some secretions this is in very minute quantity; in others, as in milk, it is larger. In these instances the separation of the colloid and its appearance in the secreted fluid are by a process of which as yet we have little knowledge.

Lastly, in the nervous matter there exists a colloidal fluid which I have succeeded in obtaining in its active state; that is to say, before it has cohered, or, as Graham would have said, become thick or pectous.

Experiment.—In some experiments conducted in 1867 on the effect of extreme cold on nervous function, and which formed a part of my courses on experimental and practical medicine, I observed that the brain of an animal could be frozen, with the result, if the process were not carried too low down into the medulla, of simply suspending cerebral function and causing deep narcotism. In birds this result was singularly striking.

In some instances the birds rendered profoundly comatose were permitted to sleep into actual death, and the brain in the frozen, or, as I may call it, *glacial*, state was removed and allowed to thaw. In the process of thawing I noticed that the brain matter passed from the hard state, due to the cold, into a soft, and I had nearly said fluid, state, and then again into a firm state, like that in which we commonly find nervous structure at our post-mortem examinations. This same series of transitional phenomena I had observed in the processes of freezing and thawing blood. Blood if frozen the instant it is removed from the vessels may be so thawed that it will become fluid, and after the stage of fluidity will pass into the semi-solid condition called coagulation; in other words, it will become pectous. Hence in watching the brain matter, as I have stated, I felt I was watching the same order of phenomena, with this difference: that the transitional stage in the brain matter seemed to me much shorter than in the blood; it was, in fact, so transient, I was unable to obtain any fluid from it to enable the change to be observed in a separated specimen of fluid.

Experiment.—By another procedure I was, however, able to study this subject in a better way. Instantly after a butcher had killed a sheep I got him to lay open the skull by one clean sharp stroke, to take out the brain from its cavity, and drop it into a vessel containing ammoniated water. The ammonia suspended the change, and, if the experiment succeeded well, a fluid could be expressed which became cohesive or pectous while under observation. To succeed in this demonstration many trials had to be made, for the brain may become instantly pectous from the vibration of the blow by which the cranium is opened. In oxen that were

felled by the poleaxe and pithed with the cane, the brain substance seemed to be rendered immediately pectous throughout its whole mass; but by the other plan named the fluid could be obtained before it solidified and by cold could be maintained in the uncoagulated state. It is the vital nervous fluid; the fluid extract from nervous matter; the circulating fluid in nervous matter; a fluid sometimes exuded; we might call it the nervous blood, as important to life and to organisation as blood itself.

By cohesive or pectous change is meant a thickening and separation of colloidal substances from a state of solution. In the colloids with which we are concerned the separation, whenever it takes place, is always from water—at least, as far as we yet know. We see colloidal matter, therefore, in two forms: (*a*) when it is in a soluble and active condition, as in liquid albumen; (*b*) when it is cohering, and is giving out water, as in clotting blood or in the formation of curd. For clearness' sake, I shall speak of these two states as—

(1) The aqueous state;

(2) The cohesive or pectous state.

Experiment.—In studying these two states, nothing is more immediately striking than the small quantity of colloidal substance that is capable of rendering a mass of water semi-solid. In a clot of fibrine removed after death from the right auricle and ventricle of an adult heart I saw the amount of colloid fibrine that once held a pound of blood in the active aqueous state.

The cause of the pectous change is one of those problems which has most baffled the physiologist. In the study of it one fundamental error has prevailed, of this nature: that every condition which allows the phenomenon to take place has been set down as the cause of it. Thus the Greek physiologists, observing

the phenomenon as it is seen in the coagulation of the blood, presumed that it was due to a process of cooling —a view still held by the vulgar, but long since overturned by Hewson, who proved that cold suspends the process altogether, while heat quickens it. But Hewson, seeing that fibrine tied up in a vein or an artery in the living animal does not coagulate, arrived at the conclusion that exposure to the air is the cause of the phenomenon. Lancisi, seeing that in an aneurismal sac the fibrine is laid down in layers, owing to the slow motion of fluid through such a cavity, was led to teach that motion maintains fluidity and that rest is the cause of the precipitation. John Hunter, noticing the disposition of the fibrine to coagulate out of the vessels, or within them when retained without motion, argued that coagulation is one of the effects of the life of the blood and is a vital operation. Scudamore, following up Bauer's original research on the evolution of carbonic acid from blood, conceived such evolution to be the cause of the pectous phenomenon. Some later physiologists, observing that the admixture of fibrine with another fluid would produce the result, have considered such admixture as the cause; omitting altogether the fact that the same will occur, under other conditions, without any such admixture. Lastly,—for I need not weary with examples,—I myself, in my early days of experiment, having observed that ammonia added to blood holds the fibrine fluid, and that blood in the act of coagulating gives out ammonia, put two and two together, not unnaturally, and therewith came to the conclusion that escape of ammonia is the natural cause of the pectous change. Had I but recalled the facts of the same phenomenon as they are seen when caseine of milk and silicic acid

become pectous, I should have undoubtedly moved my hypothesis to the second position, and should have said, as I now say, that although ammonia, in a certain proportion, possesses the power, by its presence, of suspending the process, and although when the colloid is freed of the ammonia it will become pectous, the fact, very useful in its way to know, is not an explanation of the cause of the phenomenon.

The pectous change in the colloidal substances that are capable of existing in solution in water depends upon a disturbance between the water in which they are distributed and the solid matter of which they are composed.

The colloid exists, in fact, as I have already said, in two states: the aqueous, or active; the pectous, or passive. In the aqueous state the colloidal substance is saturated with water, for which it has no affinity. It is a substance, however, possessing a strong attraction of cohesion of its own particles, and although it may be distributed through water under certain fitting conditions which enforce combination, it is ready at any moment, by virtue of the self-attraction of its molecules, to separate from the water, to adhere molecularly, and to expel from itself, when once the cohesion has commenced, the water that previously held it in solution.

In the aqueous state the attraction of cohesion resident in the colloid is resisted by the water that is present; but the resistance, in all cases feeble, is in some examples, as in the fibrine of the blood, so slight that the merest disturbance suffices to break it and allow the attraction of cohesion to commence. Give, then, the conditions for the liberation of the colloid, and the pectous change begins. The mind has only to dwell on this simple and primary fact, and all the rest of the problem is clear. Take a solution; in it distribute

plastic matter, which is plastic by virtue of its molecular self-cohesive affinity; the power of the affinity is resisted, and the colloid is held in temporary combination with water; remove the resistance, and the attraction will declare itself just as surely as will the attraction between the earth and any substance upon it, if the support which separated the substance from the earth be withdrawn.

At the same time, mere dilution with water, unless it be carried to an extreme degree, would not answer; and does not, for, although the pectous process may be retarded by addition of water, it is not prevented.

The natural plan adopted is that the colloid while in the aqueous state is held in that state either by condensation of water or by the diffusion through it, in a limited amount, of some substance which, after the manner of a salt, has the property of fixing and holding water. If this retaining or fixing body be removed on the one hand, or if, on the other hand, it be in such excess that on its own part it fixes too much water, the attraction of cohesion in the colloid begins, and therewith the pectous change; but if the saline be in such small quantity as only to fix the water through the colloid, the effect is to produce combination and to maintain the aqueous state of the colloidal matter. A simple experiment or two will suffice to illustrate this plain, yet at first sight paradoxical, fact.

Experiment.—Take blood in the fluid state, and mix with it ammonium chloride. The colloidal fibrine is present in this blood, and is retained in the aqueous state by the addition to it of the chloride.

Experiment.—In a dyaliser put a specimen of this same blood held fluid by the salt; but after having thus treated it float it upon the membrane on water.

The salt, which was the bond of connection between the fibrine and the water, will diffuse through the membrane into the water. The result will be that the blood in the dialyser will become pectous; it will form a firm coagulum.

Experiment.—Take another specimen of blood in which the fibrine has been retained fluid by the addition of nitrate of potassa. On this perform an experiment originally devised by Gulliver. Add water. Shake the water with the blood, and a change takes place: the added water distributes the combining salt, and at a given stage removes it so effectually that the particles of fibrine exert their power of attraction of cohesion, and the colloid becomes pectous. It sets or coheres as a firm jelly.

Experiment.—Reverse this process. Into a specimen of the same blood, held fluid by the potassa salt, drop an excess of potassa hydrate. This substance, from its affinity for water, will seize water so greedily that around it the fibrine will instantly assume the cohesive or pectous condition.

In these experiments a soluble salt is employed, but it is not essential that it should have been such a salt. It might have been a soluble base; it might even have been one of the acids; the only requisite is that the substance should act on water after the manner of a soluble saline. If a volatile body greedy for water be employed, the effect of maintaining the colloid in the soluble or aqueous state is the same, in a more delicate degree; for in this case the suspending agent being volatile, the colloid may be separated from the water, not only by the neutralisation of the combining intermediate agent or the dilution of it in water, but by the mere escape of it in the gaseous condition.

Experiment.—Take a solution of blood in which the fibrine is held aqueous by the mere addition of ammonia at the moment of the abstraction of the blood from the body. This blood, carefully sealed up, may remain fluid for years.

Experiment.—Take a portion of this same blood held in solution by ammonia. Spread it over a wide surface, and expose it freely to the air, so as to allow the ammonia to volatilise. The result will be that with the escape of the combining link, which in this instance is volatile, the fibrine will separate from the water and become pectous, coagulated.

But—and the facts I am about to relate make the demonstration still more perfect—the aqueous condition of a colloidal fluid in which the affinity between itself and water is held by a very slight bond of connection—as, for instance, in the fibrine of the blood—may be maintained by even simpler physical processes than the addition of a salt or analogous body.

Experiment.—I took blood fresh from the vessels of an animal, and enclosed it in a tube exposed to extreme cold, after the manner devised by Hunter. Before the cohesive change in its fibrine was possible the blood was frozen. The cohesive state was suspended because the combining water, which would have escaped as vapour had the blood been drawn in an open warm air, was sustained. The cohesion was suspended for an hour. The blood then was thawed; it became fluid; and from the fluid it passed into the cohesive or pectous condition.

Experiment.—I tied up blood in a portion of vein in an animal just dead, and at once conveyed that vein under mercury, in order to subject the blood in it to a pressure of fourteen inches of mercury, and so I left the

vein for twenty hours; then I removed it, let out the contained blood still in the aqueous form, and observed that, liberated, its fibrine became pectous in four minutes.

In this last instance the water was condensed and fixed by pressure alone. It is thus, I believe, in the living animal, that the pure colloid fibrine is held combined with the water in the blood; it is thus, I believe, that in the nervous matter the combination is also sustained. But in these latter cases, as the pressure is less than in my experiment, the pectous change is more readily developed when the pressure is removed.

Experiment.—One more experiment completes this part of my subject. If through a colloidal solution I pass a feeble voltaic current, and so decompose the water, the colloid will assume the pectous form. Put into a cup a solution of albumen, and pass through it a current from a small two-cell battery. With the slow decomposition of the water into its elementary gases, the colloid separates in the pectous form. As it is distributed in the tube in fine filaments, between which the liberated gases have infiltrated, the fibrine looks like a beautiful web of areolar tissue. The colloid separates at the positive pole.

The capability of a colloid to retain the aqueous state, in which alone it is active, and I may say vital, varies exceedingly in different forms of colloid. With fibrine the condition is so refined, the merest accident may suffice to break the aqueous connection and permit the molecular cohesive attraction. The escape of water in the form of vapour will suffice. A mechanical agitation, an electrical agitation, the agitation excited by heat—these all, even in a minor degree, will suffice to induce the same effect. The mere introduction of a nucleus or point round which the particles of a colloid

may begin to adhere will lead to precisely the same event. The aqueous colloid of the nervous matter is equally, or even more, sensitive than that of blood.

In some colloids the adhesion between the water and the colloid being closer, owing to the presence of a firmer combining link, influences more decisive are required to induce the pectous phenomenon. Albumen is an example of this form of colloidal matter.

In other colloid states the adhesion with the water may be so strongly cemented that the colloid may follow the action of water itself in becoming fluid by heat and solid by cold. Gelatine is an example of this condition of colloidal matter.

The rule is simple by which colloids in the aqueous form maintain the active form under ordinary conditions. A soluble substance acting after the manner of a salt sustains fluidity. Hence a colloid becomes more and more inclined to assume the cohesive form the more completely it is dialysed from crystalloidal matter, and the more purely it approaches the absolute colloidal character.

Experiment.—Dialysing Albumen.—Take, for example, a solution of albumen. That solution is in the perfect aqueous form, and is steadily held so by the presence in it of a combining body acting as a salt. Put a specimen of the same albumen in a dialyser, dialysing with a weak solution of hydrochloric acid. The substance that held the colloid aqueous will be removed, and the result will be that the albumen on the membrane will be left in the pectous arrangement of colloidal matter.

The same changes extend to all similar substances: to the globuline of the lens; to myosine of muscle; to a membrane.

The evidence as to the cause of the assumption of

the cohesive form from the aqueous is, I think, perfect. There is no experimental break in the chain of evidence. From the process of instant change from vibration or contact in the purest colloids to the slowest separation in the least pure, all the facts prove the truth of the proposition, and establish unity of process.

The physical character of the cohesive change is as yet obscurely defined. We have in it a certain analogy to crystallisation, but no such distinctness of detail. That which we observe in the colloidal pectous body is that it is no longer fluent, that it contracts with extrusion of water, that it solidifies with loss of volume and loss of weight, and that in its final stage of solidity it presents what may be designated the gelatinous form of matter. To what extent the extrusion of water may be carried is shown by the fact that I once found two hundred grains of pectous fibrine reduced by it to six grains.

In all cases with the shrinking and thickening of the pectous colloid there is opacity and diminished elasticity. In membranes there is rapid dryness, and with that a linear puckering. This is well illustrated in the cornea of the eye after death.

In the presence of oxygen and water the tendency of a colloid, in its pectous condition, is to pass into the crystalloidal form of matter by decomposition and re-arrangement of the elementary parts around its basic carbon. In this manner all the structure may pass into a saline form, which will then fix water, be resolved in solution and carried away.

In the animal body there is probably only one colloidal form of matter primitively. The evidence all points to this conclusion, and suggests that every apparent difference is due simply to a different mode of

combination of the primary colloid with water. In what may be called the living structures of the body the water is all colloidal; and in the form of colloid exists all the matter in nature that is capable of assuming the organised motion which we denominate life. In the brain it is the colloidal matter that is impressed by external vibration, and on which impressions are fixed. A condensed colloidal structure, the colloidal lens, is used for the refraction of the wave of light. In the muscle the motor mechanism is constructed of colloidal matter. The blood is a store of colloidal fluid. A membrane is not a mere supporter of organs, but is an active colloidal sheet possessing the property of allowing water and substances that fix water to pass through it, but detaining all that is colloidal; in this way colloidal forms are constructed into organic textures.

But that colloidal matter generally diffused may be susceptible of motion it must remain in the aqueous state. Whenever it is most vital it is most purely aqueous. In combination with saline or fatty matter it remains in some structures in a comparatively passive state. In the cohesive form it is dead, and waits either to be resolved or digested into the aqueous condition, or under the influence of oxygen to be broken up, rearranged on a new elementary plan in the crystalloidal form, and conveyed with the water from the organism. Thus it exists in the sarcous element of muscle. It has served its purpose, and waits to be resolved into urea.

In a few instances pectous colloidal of matter remains on the body, fulfilling a useful purpose as dead protective matter. In the hair, in the nails, in the epidermis, it is present for such intention.

COTEMPORARY PRACTICE AND LITERATURE.

"*Every physician will and ought to make observations from his own experience; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together.*"—FRIEND.

THE MEDICAL ASPECTS OF THE OPIUM QUESTION.

IN the month of May last two medical meetings were held, one in Calcutta on the 11th and 18th of the month, the other in London on the 31st, to consider the medical aspects of the opium question. Over the meeting in Calcutta Dr. Koylash Chunder Bose presided; over that in London I had the honour of presiding, at the invitation of the Society for the Suppression of the Opium Traffic. At both meetings the one subject discussed was the effect of opium on the human constitution from a medical point of view, and in both the readers of papers were military medical officers in the Indian service, thoroughly qualified to form a judgment from direct personal observation and experience. The contradictory nature of the observation and experience thus supplied forms a singular chapter in medical literature.

At the Calcutta meeting * the discussion was opened

* Supplement to the *Medical Reporter*, Calcutta, edited by Laurence Fernandez, M.D., August 1st, 1892.

by Surgeon Lieutenant-Colonel A. Crombie, M.D., who took up the defence of opium-eating and compared the practice most favourably with the custom of drinking of alcoholic fluids in European society. He related the fact of a man in India who ate nine ounces of solid opium daily "without its developing any poisonous symptoms whatever." In India to "eat opium together" is an inviolable pledge of a bargain. On a birthday, when Rajput chiefs convive, they drink health from a bowl of water in which a lump of opium has been stirred. Opium is taken also because of the exhilaration it induces. In short, it is in India the counterpart of alcohol in Europe and America. Like alcohol, it is also taken "to chill the keen edge of the mental perceptions and the physical sufferings. Probably 90 per cent. of the opium-eating peasantry of Bengal begin to take opium on account of its power to relieve some physical suffering. Chronic rheumatism is the most common cause, and after that chronic diarrhœa and dysentery, and in certain instances malarial poison. It is also taken to modify the discomfort of insufficient clothing and to allay the pangs of hunger." It fulfils, according to Crombie, the uses looked for in these respects so well, "one is often surprised that opium is not more often habitually taken than it is." Dr. Crombie supported his argument by quoting the views of Dr. Cayley and the late Dr. Vincent Richards. He contended, from these authorities, that opium sustains endurance, and that most opium-eaters are induced to begin the habit through disease. Turning to the effect on the constitution of the habitual use of opium, he opposed the views of the late Sir Robert Christison as to the effect of opium-eating in Turkey and Persia, and was astounded at five thousand practitioners of

medicine in Great Britain—every one of whom is too familiar with the tissue changes produced by alcohol—signing a petition against the dietetic use of opium in India. They had, he said, no trustworthy evidence before them, and he averred that the "prematurely senile and decrepit opium-eater is the exception, and not the rule, and that the rule is rather a hale, hearty, and respected old age." In a final section of his address Crombie compared opium-eating with alcohol-drinking as a cause of insanity and of crime, and from the statistics of insanity he deduced "that as a cause of insanity, and as compared with alcohol in Europe or even in India, opium is literally not in it." In relation to crime and to deterioration of the mental faculties by opium-eating and opium-smoking, he contrasted the opinions of Sir William O'Shaughnessy and of Dr. Waring, the first of whom saw little evil in the practice, the latter much; and he then related his own independent inquiries in opium bazaars where smoking as well as eating opium is practised. The particulars were singularly interesting, and the conclusion arrived at was "that the opium-eater is neither quarrelsome, silly, nor sentimental, nor aggressive; he does not assault his wife, nor do anything which calls for the interference of the police. He is nobody's enemy but his own; neither does the habitual use of opium give rise to any condition comparable with the delirium tremens of alcohol."

Dr. Crombie's observations were followed up by those of the President, Dr. Koylash Chunder Bose, Dr. Jogendra Nath Ghose, Dr. Moir, Dr. Ram Moy Roy, and other members. Finally, a summary of all the opinions expressed was drawn up by Crombie, and included sixteen heads, the essentials of which run as follows:—

"That opium-eaters constitute from 5 to 10 per cent. of the people of Bengal, and in certain parts of India the proportion is much larger; that in some parts opium assumes the position in social life and ceremonial so long granted to alcohol in other countries; that the habit is usually contracted as a prophylactic; that the small daily use is a distinct benefit in some diseased conditions, in diabetes specially; that the average daily ration of Indian opium (which contains 1·5 to 7 per cent. of morphia, as compared with 10 to 21 per cent. of Turkey opium) is from seven to twelve grains; that the moderate daily consumption of opium is compatible with good health, bodily and mental, and does not obviously tend to the production of disease; that the most usual ill effects of the habit are hebetude, lassitude, indifference to external impressions, timidity, a tendency to neglect business, diminution of the birth-rate, and when the quantity consumed is large emaciation, disorder of the bowels, and early death from some intercurrent disease; that the habit is sometimes an indirect incentive to pilfering and petty theft, but even taken in excessive quantities it does not lead to perceptible tissue change, to grave or violent crime, to insanity, to infringement of public decency or order, or, frequently, to domestic unhappiness; that the habitual use of even large quantities (a drachm and upwards daily) is often compatible with the enjoyment of excellent health, the keen and successful pursuit of business, and an honoured old age; that the morphia habit is extremely rare in India and is confined to large towns; that one morphia *habitué*, a pleader, carried on his business for many years on a daily ration of ninety grains of morphia; that the reduction of an opium-eater's daily ration is neither difficult nor infrequent;

that 47 per cent. of opium-eaters contract the habit when between forty and fifty years of age; and that of 2,202 admissions of patients into the lunatic asylums of Lower Bengal 641 were ganja (cannabis) smokers, 117 spirit-drinkers, and 8 only opium-eaters, the figures from other provinces of India showing similar results."

In the above abstract I have given the pith of the Calcutta debate. It represents statements drawn up on the field of observation by men of great competency, but one of whom at least indulged in the opium habit.

Let me now put in contrast the views of the members of the London meeting over which I presided.

The report of the London meeting* on this same question presents a very different aspect. The proceedings were opened by Dr. James L. Maxwell, who dealt with the opium habit as it is now practised in China. He explained that 70,000 chests of Indian opium, representing 10,000,000 pounds weight of the drug, are sent annually from Calcutta and Bombay to China; whilst 50,000 ounces of morphia are exported into the same country each year. Over all China one and a half drachm daily is a moderate estimate of the opium smoked, and 20,000,000 to 25,000,000 is the ordinary estimate of opium-consumers in the Celestial Empire. The habit assumes three forms: smoking opium; eating opium; swallowing morphia. The effect of the habit of smoking opium, according to Maxwell, is to produce emaciation of body, impaired digestion, and diminished vitality. Opium, he urged, is no prophylactic against malaria, and is no protection against famine. It may take off the edge of hunger, and the weakened

* *The Medical Aspect of the Opium Question: a Report of a Discussion on May 31st*, 1892, published by P. S. King & Son (London, 1892).

vitality of the opium-smoker may enable him to do with less food, but it will not enable him to live on roots and bark, as the non-smoker will contrive to live. He affirmed also that the habit causes moral degeneration, and referred to the vast army of suicides which its con stant use so readily produces.

Dr. Donald Morrison, of Bengal, in the next paper read, took also the offensive against the views that had been authoritatively expressed in favour of the opium habit. He refuted the theory that opium is prophylactic; and he quoted the evidence of one of the ablest physicians who has ever made the subject a careful study, Dr. Russell, one of the medical officers of Patna, to confirm his argument. Dr. Russell's evidence is so remarkable, it is worthy of special notice. He writes, "The opium-eater enjoys considerable immunity from malarial affections in the early stage—the first few years of indulgence in the habit—before the organic visceral changes are set up, *and the general shattering of constitution results, which prematurely breaks down the consumer of opium and renders him an easy prey to diseases of every kind.*" . . . . In his district (Gauhati, Assam) Russell has made a series of exact observations on the prevalence of the opium habit among the large circulating population of the jails. He finds that nearly four-fifths of the men of the plains who enter jail are more or less addicted to this habit, consuming 5 grains to 180 grains of the drug daily, and that the prevalence of the habit is the curse of the jail populations in Lower Assam. No work can be got out of the long-confirmed opium-eater. He can digest nothing but light food, milk, or soups; on ordinary diet he suffers from diarrhœa, tending to run rapidly into dysentery. His system has very slight heat-making power; he is extremely

susceptible to any changes of temperature, and cannot stand cold; he is thus especially liable to both chest and bowel disorders. Again and again he may be nursed by a system of milk diet, gradually on to ordinary food; again and again he returns to hospital, suffering from diarrhœa, dysentery, or dyspepsia.

"The emaciation of the opium-eater is characteristic and extreme. Eventually, after having been a source of infinite care, after repeated courses of medical and dietary treatment, after having caused large expenditure in sick diet, extras, etc., he perishes, usually of a chest or bowel disorder, or, perhaps, from practical starvation from eventual inability to digest any kind of food, even the lightest and most delicate. On post-mortem examination, all the viscera are usually found wasted and anæmic except the liver, which is commonly large, pale, and very fatty." These quotations from Dr. Russell's book on *Malaria and Injuries of the Spleen* refute completely all that has been written in support of the harmlessness of the opium habit. Even if opium did protect the *habitué* from malaria in the early stages of its use—which is far from being the case—yet to advocate its use and continuance in order ultimately "*to render him an easy prey to diseases of every kind*" is surely a questionable benefit, and especially when "*he perishes usually from chest or bowel disorder, or, perhaps, from practical starvation from eventual inability to digest any kind of food.*"

Other speakers followed, including Deputy Inspector-General Partridge, Dr. Edward Horton, and Brigade Surgeon Pringle, M.D., who held views similar to those of Dr. Maxwell. They were opposed by Surgeon-General Murray, who held that opium as generally taken soothed pain and uneasiness and promoted digestion;

and he affirmed that there was a wonderful unanimity of opinion in India and China that opium contributed much to the comfort and happiness of life when it was used in moderation. He thought also that the human race required some species of excitement or indulgence, and that the stoppage of the supply of Indian opium to China would stimulate greater production there, a result which would ruin India for the benefit of China.

These are the difficulties of this great contention. As chairman of the English meeting, I submitted what I may here repeat as a reviewer :—

The course of the debate has led us to five points. (1) There are some who say that opium may be used in moderation and without danger. (2) There are others who say that it cannot be used in moderation without danger. (3) There are some who believe that opium is a prophylactic. (4) There are others who, on startling evidence, declare that no prophylactic action belongs to it. (5) All agree that if there is anything like an excess in the use of opium, whether by smoking it, or injecting it under the skin, or taking it by the mouth, there is danger.

I have no such knowledge of the effects of opium as that which has been presented to us in the able papers to which we have listened. My knowledge of it has been confined to England. Some years ago I took a great interest in the opium dens of London, and I visited them for the purpose of ascertaining whether there was any distinctive difference, with regard to the symptoms produced, in smoking opium in those dens as compared with the taking of opium in the solid state or the injection of it as morphia. I was a witness to Lascars lying under the influence produced by opium-smoking in one well-known den, and I have had

experience for many years with regard to persons who take opium by the mouth or subject themselves to the injection of morphia. Many people who have come to me have been the very opposite of those who have been spoken of by Surgeon-General Murray. All who have come to me have been rich enough to come to a physician in London, and, generally speaking, that does not mean poverty.

"From these experiences I gather that there is a distinctive difference betwixt smoking opium and taking it by hypodermic injection, or swallowing it. There are distinctions of a very remarkable kind. I do not mean to say that smoking opium is less injurious than injecting it, but the symptoms are different. The persons who take morphia by injection, or who swallow opium, do not immediately go to sleep as those who smoke opium do. What I observed always in the opium-smoker was that he had a tendency to go to sleep; and, in fact, in the opium dens, where the men lay upon sackings, stretched across as a bed, there would be half a dozen lying at one time smoking themselves to sleep; and they would sleep until it was time for them to be awakened that they might get back to their duties. They slept all the time. I was anxious to ascertain whether the temperature of the body fell under these circumstances of deep sleep, and I took a thermometer to get the temperature of the body; but I was warned that the responsibility of pushing the inquiry so far must not be accepted. I noticed the fact that there was in the appearance of these men the peculiarity that the secretions were checked. The skin in many of them was like leather, dry, hard, and shrivelled; and I understood that the internal secretions were much suppressed. I have never seen these

facts so strikingly in opium-eaters, or in those who inject morphia. Those who inject do not go to sleep directly. In fact, they go about their duties in the most remarkable manner for some time after they have been injected. I have known a man, filling an important office, retire for a period from a public meeting, go into a quiet corner, inject himself with morphia there, and return to his public work; and I have seen a member of my own profession, while denouncing heartily the habit of injecting morphia, deliberately take a bottle out of his pocket and inject himself in my very face, as if he had forgotten all about the abuse of the drug, and as if injection had become an automatic act which he must perform. These differences are marked. There is, then, a physiological difference between the smoking of opium and the taking of it by injection or swallowing. But of this I am quite sure: that in neither is there any benefit. I am not sufficiently conversant with the smoking habit to say that it is better or worse than the other, but I am certain with regard to the morphia habit, and with regard to the taking of opium by the mouth, that, whether the drug be taken in the most moderate form or in a larger quantity, the difference of mischief is only one of degree.

I never saw one opium *habitué* of all who have come to me uninjured by the habit. I have known some who have taken only a small quantity—perhaps from three to six grains of morphia a day. I have known another who has run the dose of morphia up to ninety-six grains in the day—ninety-six grains of *morphia*; but who has come down to four grains of morphia a day, and if I insisted that it should be stopped altogether, I should not get what I wanted. But neither moderate nor

immoderate *habitués* are the better for what they take. There is uncertainty about their action; there is uncertainty about their word; and there is a danger at any moment of their running into excess—of one going back to the large dose, and of the other increasing the dose to the extent of doing real mischief.

We should bear in mind, in studying the effects of this drug upon the human body, that it produces a desire for itself. Like alcohol, chloroform, or ether, opium and morphia produce a desire for themselves, and when once they begin to produce desire, they produce, in degree, a new constitution, with the tendency to cry for 'more,' until excess becomes danger.

Is there any good at all, under any circumstances, in the habitual use of opium by any mode? I never could see that there was the slightest good—never, except medicinally. When opiates are prescribed for a few days medicinally, there may be great good; but why a person should become habituated to them, in however small a quantity, I never could see reason. What does opium do under such conditions? Does it increase the muscular power? No. Does it maintain the natural temperature of the body? No. Does it improve the appetite? No. Does it assist the digestion of food? No. Does it relieve the mind from any peculiar weight that may be upon it? No. Does it whip men up to become more active and useful? No. We ask all round, point by point, what it does. We find, after all, that it does nothing but sustain an absurd habit, introduced into the world for no useful purpose whatever, and which, continued until a new constitution declares it necessary, inflicts infinite evil.

OPERÆ PRETIUM EST.

FOUR works deserving specially to be included under the above title lie before me this quarter, and it would indeed be difficult to decide which is of most value. In their respective departments they stand alone.

TUKE'S "DICTIONARY OF PSYCHOLOGICAL MEDICINE."

DR. HACK TUKE'S *Dictionary* (J. & A. Churchill) is in two volumes of handsome build and full of matter to overflowing. One hundred and twenty-eight authors contribute their labours, and every subject connected with psychological medicine is brought under review up to the latest date. The *Dictionary* bids fair to become historical even amongst dictionaries, and it is but just to its learned author, Dr. W. C. Coupland, to say that the opening part, entitled "Philosophy of Mind," is a splendid effort. It will be news to many of the most experienced critics and writers of modern medicine that we have amongst us so competent a hand in this department of knowledge.

I do not attempt to criticise or even analyse any one of the articles in this new dictionary. It must be accepted straight off the reel as a standard work, essential not only to every medical man's library, but to every man's practice who has a practice worthy of the name. The subject matter of the book bears on insanity and the treatment of the insane as the first consideration, but there are important points in it of medical jurisprudence, and of practice of medicine also, which make it invaluable as an A B C of medical learning on mental disease and mental function. The

name of Tuke has long been known in connection with the treatment of the insane, and Dr. Hack Tuke has now set his seal to a new lease of it for a term of usefulness and fame extending far beyond the generation in which his effort has been conceived and brought forth, and far beyond the medical circle for which it is more immediately intended.

THE "ANNUAL OF THE UNIVERSAL MEDICAL SCIENCES."

DR. SAJOUS'S fifth series, in five volumes, of the *Annual of the Universal Medical Sciences* (F. A. Davis & Co.), is to hand, and in every respect crowns those which have gone before it. The writers of the different sections are now gaining the experience which tells them what to introduce into their pages in the briefest and yet most useful and practical form. It were invidious to compare one writer with another where all are so good and conscientious; so I open the first volume of this series at a venture, and alight on Section C, a report in forty pages on diseases of the mouth, stomach, pancreas, and liver, by Dr. Solis Cohen. Here is the pith of the year's work of universal medicine in that one department, told in the easiest and yet soundest style, a style typical of the other sections. It is not books or monographs alone that are reviewed in the *Annual*, but papers in the many periodicals, and discussions of the learned societies. The success of the *Annual* depends largely on the application by the distinguished editor of the old principle, "divide and conquer," but the enterprise of the publishers should not pass without recognition. The *Annual* is a great undertaking

commercially, and deserves to win its way wherever medicine is scientifically cultivated.

WATTS'S "DICTIONARY OF CHEMISTRY."

THE third volume of this comprehensive work, by Dr. H. Forster Morley and Mr. M. M. Pattison Muir, is now issued by Messrs. Longmans. It extends from "In" to "Ph," and embraces 856 closely printed pages. The article entitled "Theories on the Molecular Constitution of Bodies," by Professor J. J. Thomson, is excellently written, and makes an abstruse, because as yet theoretical, subject as plain as such a subject can be made. The article on "Nitrogen and Compounds of Nitrogen," by Mr. Muir, deserves careful study, as does the "Nitrogen Group of Elements," by the same writer. Mr. Shenstone contributes the communication on "Ozone," and Professor Haliburton the articles on "Milk" and "Muscle." The *Dictionary* is so valuable an addition to the medical library, no such library is complete without it. In it the publishers have undertaken an enterprise deserving the best thanks of the scientific world.

QUAIN'S "ELEMENTS OF ANATOMY."

VOLUME II., Part II., of Quain's *Elements of Anatomy*, edited by Professors Schäfer and Thane, has been issued by the same firm (Messrs. Longmans). I have noticed the previous parts of this important work with admiration, and in the same spirit every one who reads it must accept the present part, by Professor Thane. The volume is illustrated by 225 engravings, many of which are

coloured. Professor Thane has the well-earned credit amongst young students of being one of the most lucid teachers and demonstrators of anatomy, and if an old student may venture on such an observation, he deserves the same credit for his teaching with the pen and the pencil. The volume is really as valuable to old as to young students, and there is not a practitioner anywhere who will not feel his work strengthened by having the volume always at hand.

COLLECTANEA.

AMONGST other works most instructive in their quality may be named Eisenberg's *Bacteriological Diagnosis*, translated by Norval H. Peirce, M.D. (Davis); Lauder Brunton's *Introduction to Modern Therapeutics* (MacMillan); McKendick's *Life in Motion*, lectures delivered at the Royal Institution last Christmas (A. & C. Black); Wethered's *Medical Microscopy* (Lewis's Practical Series), a capital student's manual; Eminson's *Epidemic Pneumonia in Scotter* (H. Kempton), an essay in which the author treats of pneumonia as produced by emanations from foul sewers and soils; Blackwell's *Why Hygienic Congresses Fail* (G. Bell & Sons), a skilful and thoughtful answer to an all-important question; Roger Williams's *Observations on the General Pathology of Cancer*; and an elaborate report by J. Ashburton Thornton on *Influenza in New South Wales*. It is also pleasant to welcome a new and able medical monthly, *The Medical Magazine*.

CHOLERA LITERATURE AND PRACTICE DURING THE LATE EPIDEMIC.

IF the literature that has been written during the past quarter on the subject of cholera were to be put together and published, it would furnish with books a fair-sized library. Writers of all classes, journals of all kinds, have contributed to it; Governmental authorities have sent in their quota; doctors innumerable have added their mites; and, the Royal College of Physicians of London, at the request of the Local Government Board, has written its prescriptions, as it once before did, for those remedies which it thinks most advisable to meet the requirements in view should the disease make progress in the United Kingdom. It has been a very instructive task to watch the course of this literature, whether general or medical.

THE GENERAL LITERATURE.

By comparison with previous periods of literature composed under similar circumstances, the general form of communication and recommendation has been of a superior order. As a necessity a great deal of nonsense has crept in here and there; but taking it altogether, the general literature has stood out as soundly as the medical. I select as an illustration of the general kind, because it is nearest at hand, the following

thirteen simple rules published by the *Daily Graphic* of September 3rd. They are typical of what appeared in other leading public journals, and are reprinted here that they may be preserved in a medical work as good models for professional men who may be consulted, at a moment's notice, on the immediate issue of instructions to the people. They were sent to me before they were published, and, at the request of the editor, I supplemented them with a few details, but in truth there was little to be added to, and nothing to be taken away from them. I know not who drew them up; but, whoever he was, he knew his business and how to arrange his material so as to give proper position to each rule.

"A Few Simple Rules.

"1. Boil all suspected water before drinking.

2. Do not drink such water more than twenty-four hours after boiling.

3. Do not trust to filtering.

4. The dangerous qualities of water are not obviated by the addition of wine or spirits.

5. See that all water-pipes and cisterns are kept clean.

6. Boil all milk immediately upon its reception in the house.

7. House refuse should immediately be cleared away.

8. Thorough washing and lime-washing of uncleanly, and especially of densely occupied premises, should be frequently done.

9. Be careful not to breathe impure air or air vitiated by odours from sewers, house drains, and cesspools.

10. See that no leakage of impure air arises from the closet or the sewer pipes.

11. See that your cistern waste pipe is cut off from the sewer pipe.

12. Use the following disinfectants: for floors or soiled linen, corrosive sublimate; for large masses of filth, sulphate of iron; for unoccupied rooms, sulphurous acid gas.

13. Should any inmate of the house be attacked, all excreta and soiled linen should be immediately disinfected as above.

Measures of cleanliness taken beforehand are of far more importance for the protection of a district (or a house) against cholera than removal or disinfection of filth after the disease has actually made its appearance."

If in previous outbreaks of cholera rules like these had been largely published the disease would certainly have been quickly reduced even when it had taken hold of the community.

The grand advance which all the important journals seem to have made is the acceptance of the theory that cholera is carried by water, and that the cause of it is swallowed. This was the theory enunciated by the late Dr. John Snow. It has required thirty years for its demonstration, and it stands now as firmly as it ever did. With it is coupled the understanding that cholera is not a contagious disease in the same sense as diphtheria or small-pox is; that is to say, it does not pass from person to person carried by the air, but follows the lines of human intercourse borne by particles which adhere to clothing and to the bodies of the affected, and which infecting water, renders the water poisonous and productive of the disease in those who drink of it.

There can be no doubt that the knowledge of this

theory has had a good effect. It has prevented panic; it has removed mystery and the superstition that breeds mystery amongst the people. The layman now feels that he knows where the cholera lies, how to keep himself free of it, and how to assist in preventing the spread of it amongst others instead of running away from it in sheer funk and despair.

THE MEDICAL LITERATURE.

The medical literature on the subject has unfortunately been contradictory, and, in an age so critical as the present, has been exposed to a considerable amount of criticism. The report of the Royal College of Physicians has not even escaped. Intelligent people in discussing the College report think it indecisive and wanting in clearness. The report is included in seventeen heads, and is remarkable in that it does more than was wished for by the Local Government Board. The Local Government Board asked for prescriptions in emergency, nothing more; the Royal College puts, in foremost place, rules of prevention, some of which, it is complained by readers, suggest doubts. Rule 5 is of this kind:—

"5. As water is one of the chief agents by which choleraic infection is conveyed, all water employed for personal and domestic use in the household should be scrupulously protected from contaminations of every kind, and if any doubts of its purity arise, the water should be boiled, filtered, and consumed within twenty-four hours. Boiled and filtered rainwater is probably the best of all waters for use at this time."

The rule seems to convey the idea that water is only one agent by which the choleraic infection is conveyed, and hence the query what are the other agents and

what the plan for avoiding danger from them. Again, how are doubts as to purity of water to be settled by ordinary people, and what kind of water is it that need not be boiled? Once more; in the opening sentences it is stated that "the College proposes no instructions for the treatment of cholera. Every case requires separate consideration and management." But in the rules running from 10 to 16 elaborate instructions are given for the treatment of choleraic symptoms in their early stages. In the first place, there is a series of rules, 9 to 12, giving general directions for the management of looseness of the bowels coming on during the practice of very cautious methods of life, especially in regard to the use of stimulants of an alcoholic kind.

"9. If, notwithstanding this careful regulation of the manner of living, looseness of the bowels should set in, send immediately for medical assistance, since, without personal examination and direction, no case of this kind arising in such circumstances can be satisfactorily or even safely managed. But if medical assistance is not immediately available, follow the subjoined instructions until the doctor arrives:—

"10. Choose, if practicable, a bright, airy room, go at once to bed, keep quite warm, and if troubled with cramps or pains, apply hot applications to the entire stomach.

"11. Take freshly prepared fluid or semi-fluid food in quantities of a large cupful at a time regularly every three hours. Such food may consist of boiled milk, thickened with rice flour, baked flour, or biscuit powder; of tea made with boiling milk infused about five minutes, and having toast, biscuits, or rusks soaked in it; of farinaceous puddings of the nursery sort; of any kind of gruel, except that made with coarse oat-

meal; of meat jelly; of beef tea; or of mutton, chicken, or veal broth.

"If pain persists with depression or faintness, take a tablespoonful of brandy or of whisky in a small claret glassful of hot water after meals twice, thrice, or four times in the course of the twenty-four hours, but not oftener than is absolutely required for relief.

"12. If thirst becomes excessive, sip from time to time small quantities of iced water just sensibly acidulated with fresh juice of lemons or with aromatic sulphuric acid."

These general instructions are followed by others purely medical:—

"13. As soon as possible after looseness of the bowels has begun, take in capsules, or in hot milk, or in any other manner preferred, two teaspoonfuls of castor oil. If when the action of the oil may be fairly supposed to have ceased the looseness increases to a watery diarrhœa, let the hips be well raised and carefully inject into the bowels a quart or more of hot water containing two drachms of benzoate of soda or thirty grains of tannin. Furthermore, if there be much pain in the bowels, fifteen to thirty drops of laudanum may be added to the injection. The injection should be retained as long as it is comfortable to the patient, and it may be repeated once or twice daily during the continuance of the diarrhœa and until medical assistance has been procured.

"14. After the administration of the injection, if one has been found necessary, the following mixture should be taken at intervals of from three to four hours, according to the urgency of the symptoms."

There are then supplied prescriptions for two kinds of mixtures. The first is the old "chalk mixture,"

very similar to the form published by the same authority in a previous epidemic, and which Chadwick testified was of great service if administered on the spot when the first symptoms of choleraic disorder presented themselves. The prescription includes an ounce of compound chalk mixture, half a drachm of compound tincture of camphor, half a drachm of compound tincture of chloroform, twenty minims of aromatic spirit of ammonia, five grains of salicylate of bismuth, and ten minims of essence of peppermint, the whole to make one dose, to be taken every three or four hours.

Should this mixture disagree or in four-and-twenty hours fail to give relief, another mixture is prescribed, containing as its principal ingredients fifteen minims doses of aromatic sulphuric acid, half a fluid drachm of compound tincture of camphor, twenty minims of compound tincture of chloroform, twenty minims of syrup of orange flowers, with peppermint water to make up a fluid ounce. The dose to be taken every three or four hours.

"16. From the first appearance of looseness of the bowels the body should be washed with warm water night and morning, and quickly dried; soiled bed or other clothing should be immediately disinfected and destroyed.

"A cheap and efficient disinfecting fluid is recommended by Dr. Thorne Thorne, and is thus prepared:— Dissolve half an ounce of corrosive sublimate and five grains of commercial aniline blue in three gallons of water, and add thereto one fluid ounce of hydrochloric acid. Preserve in earthenware jars or wooden tubs."

The objection offered to these medical rules by many of the public, and by some of the profession, is that they are not sufficiently precise; those who have treated cholera think also some of them would be found

difficult in practical application. They leave to the discretion of the people the selection of two remedies for the injection into the bowels, namely, benzoate of soda and tannin. But how should the selection be made? These are different remedies; how choose one? Why not give both, one as an antiseptic, the other as an astringent? The water is to be a quart or more; how much more? The water is to be hot; how hot? The injection is to be retained, but what if it cannot be retained? Is it then to be repeated, and how often? If pain persist with faintness, a tablespoonful of brandy or whisky is to be given three or four times in twenty-four hours, but not oftener than is absolutely required for relief. The people say on this, What brandy, what whisky, shall we select? for they are treacherous remedies, very likely to be impure. What also if they on being taken seem to lull the pain without stopping it, and increase the desire for more of the remedy? Shall we go on with the remedy under those circumstances? As to the two mixtures, what are the indications that they ought to be changed? When should the first be given up, and the second commenced?

These are the common objections raised in respect to the rules by the people, or, perhaps, I had better say by critical lay inquirers respecting them. Members of the profession who have seen cholera and have treated it may be disposed to be critical on two other points: they may ask, On what sufficient evidence is the administration of castor oil in choleraic cases based, and why is it recommended that the first mixture of an astringent kind is to be tried for twenty-four hours? Twenty-four hours is a fair period for cholera to run its whole course. Hundreds seized at first with very mild symptoms of it die within twenty-four hours. In

asking this, no objection is taken to the mixtures, for in both opium is the sheet anchor. But why two?

It was a pity, perhaps, that the Royal College was called upon to prescribe at all. The task must have been a most delicate and unsatisfactory one to the authorities. The rule "So many men, so many opinions" must prevail even within the College walls, and out of them it is dominant, for there are many other colleges and collegians not one of whom has an absolutely fixed and uniform idea as to the basic treatment of the great scourge. Every instruction therefore publicly offered must be tentative and in the form of compromise.

In other medical literature bearing on this latest epidemic of cholera, there has appeared, up to this time, no new suggestion that can be considered of weight either in respect to cause, symptoms, mode of death, or treatment. In Hamburg, where the disease has been most displayed to the medical fraternity, it has appeared too rapidly and destructively to admit of being studied with the calmness and precision necessary for advancing research; and if any new point has been made we must wait for its announcement.

THE SANITARY LITERATURE AND PRACTICE.

The sanitary literature as applied to sanitary practice has been sound and satisfactory. Under the leadership of Dr. Thorne Thorne, the wisest and promptest provisions have been made for preventing the introduction and diffusion of the disease into England. To his reports we are deeply indebted, with an unbounded gratitude for favours yet to come. To the practical energy of Dr. Collingridge, the able and courageous medical officer of the Port of London, we owe also a deep debt, with a similar hope, for with the sanitary

business must come up, in time, the whole of the vexed question of quarantine, on which he will be able to speak with signal effectiveness.

As a set-off, however, to the excellence of the sanitary rules best adapted for practical purposes, and which have been so sound and useful, the statements that have been ventured bearing on the theory have most significantly distorted and falsified history. Writers, most writers, are at this moment so infatuated with the germ speculation applied to disease, that they have ignored all research previous to the period before it was advanced, and have actually attributed the success, so far achieved, in warding off cholera to the knowledge which this hypothesis is supposed to have revealed. The proceeding is as foolish as it is incorrect. The water theory of cholera, and all wise precautions against the spread of cholera, were known, described, and acted upon ere ever the germ notion was in evidence. In 1853-54 we knew as fully as we know now that cholera travels no faster than men travel; we knew that it followed lines of human intercourse; we knew that it seemed to travel through the medium of water, and that it was swallowed, not inhaled; we knew that, in the common meaning of the term, it was not contagious from one person to another, and that persons who kept themselves scrupulously clean were not in the same danger from it as other people. We knew that cleanliness was the grand preventive. More we did not pretend to know, and we left ourselves open to inquire further—a good and safe position. Now, seduced and hampered by this germ analogy, the many assume to know all, whilst they know nothing positive whatever. A bacillus called the "comma bacillus," found frequently on the alimentary surfaces

of the human subject under quite ordinary circumstances, has been found in cholera excretions; therefore this bacillus is the germ of cholera, its increase the cause of the symptoms. The assumption, at this stage of inquiry, is preposterous. How does such a bacillus produce the nervous phenomena with death often in a few hours? Why do the effects of it come and go? Why, if it be the cause of these effects, can it not be destroyed even in the human body? The solemn truth is, and hundreds of us in medicine know the fact, this germ speculation, based on the hazardous grounds of analogy and coincidence, has proved as little as it has hindered much. It has led men from the beaten track of medical research and advancement, the track of ages, and has enticed them deeper and deeper into the slough of doubt the farther they have followed it. The very *ignis fatuus* of modern medicine, it has never done more mischief than in the study of cholera in the latest march of that disease through the world.

TO CORRESPONDENTS.

In order to prevent the frequent and troublesome mistake of addressing communications to the "Editor," it is necessary to repeat that everything published in the ASCLEPIAD *is exclusively by the Author whose name is on the title-page.*

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The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

September 15th, 1892.

Asclepiad No. 36 Vol. IX. Fourth Quarter 1892.

THOMAS SYDENHAM. M.D.

Autotype from an engraving by Goldar, after a painting by Sir Peter Lely, in the possession of John Sydenham. Esq.

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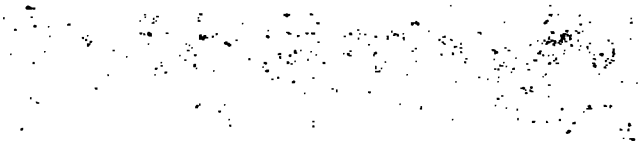

THOMAS BYNEHAM, M.D.

ON THE CAUSE AND PREVENTION OF DEATH FROM CHLOROFORM.

LECTURE III., CONCLUDED WITH COMMENTARY.*

THE TREATMENT OF CHLOROFORM COLLAPSE; RESUSCITATION.

WHEN, unhappily, fatal signs occur in the human subject from the inhalation of chloroform, there is usually a rush of attempts to call back the phenomena of life. One observer will flip the motionless body with a towel, or briskly rub the limbs; another will "try galvanism"; a third will commence artificial respiration; a fourth will combine every method. In a few cases there have been recoveries from seeming death during the application of some particular plan; thereupon that plan has been vaunted as the plan, it has been tried again, and has not succeeded, and then it is urged that, in the case where it seemed to succeed, it was merely coincident with natural recovery.

On the whole, nothing whatever has been learned from the human subject as to the best procedure after apparent death from chloroform, nor is it likely that anything can be learnt in this manner. In the first

* Delivered April 5th, 1870.

place and fortunately, no individual man can attain a sufficient experience from the human subject for systematic guidance; secondly, a research made for the mere sake of research on the human subject accidentally destroyed by chloroform would be unjustifiable; and thirdly, the excitement and anxiety of attendants on those who die is so intense that no rigorous observation or criticism of treatment can be carried out. We have, therefore, to learn all that is to be learned from the inferior animal, and in this way we can learn with hope for the future, and with soundness of observation at the time; we can repeat observation on observation; we can prove what is injurious in treatment; we can note down what is useful; we can confirm what is real. In this research, moreover, while the object we have in view is one of the greatest and most humane that can occupy the human intellect, it can be carried out without inflicting on the inferior animal world any trace of suffering. The animals simply sleep as we do when we inhale the narcotic vapour.

This, therefore, has been the inquiry pursued by me in respect to methods of recovery from death by chloroform, and I may say at once that much has been learned that can be demonstrated as useful.

There are two methods deserving consideration in respect to resuscitation; one may be called the natural, the other the antidotal, method. By the first we aim to sustain the organic functions so effectively that the poisonous vapour may be lifted out of the system, and the organic functions be therewith restored. By the second we aim to neutralise the poison, or to counteract its effect, by the employment of some opposing agent or principle.

In pursuit of the natural method I have studied

many plans: artificial respiration; electrical excitation; artificial circulation; transfusion. For full particulars of these researches I must refer you to my papers in the *Proceedings of the Royal Society* and in the *Medico-chirurgical Review* for July, 1862. For the present moment I confine myself chiefly to two of these methods: artificial respiration, and electrical excitation. These are the two methods best known and most commonly used; and if I define clearly their values I shall not only leave some practical facts on the mind, but shall lead up to future advances towards more accurate knowledge and experience.

On the human subject artificial respiration is followed out simply in order to imitate natural respiration; while galvanism is followed, in a less definite manner, by some to excite respiration, by others to excite the heart, occasionally for neither of these reasons or for both. In my researches on lower animals I have used artificial respiration for the same simple purpose as it is used on the human subject, striving only to make it perfect, while I have used galvanism for three distinct purposes: first, to excite and sustain respiration; second, to excite and sustain the heart and respiration; third, to excite the heart while the respiratory process was sustained by artificial respiration.

You will ask immediately, Can anything be done? I answer immediately, Yes. I answer that in some cases where death from chloroform appears to be perfect —where, if nothing were done, the animal would be dead—life can be restored, can be restored with certainty I may venture to say, without fear of saying more than is true, in one case out of three. We find at the same time that the result is not the same after each of the four modes of death by chloro-

form—namely, (1) by apnœal syncope; (2) by epileptiform syncope; (3) by cardiac paralysis; (4) by shock. It is most easy after the first mode, less easy after the second, and rarely possible after the third. I have not tried it after the fourth, but I should infer that it would be nearly the same as after the first mode. What is more—and the fact is as singular as it is obvious—the restoration of life is much more easy during cold and cool than it is during hot weather. When the temperature of the day is between 70° and 80° Fahr. the experiment usually fails. Lastly, the experiment usually fails if the chloroform be administered for several minutes before the narcotism passes into death.

ARTIFICIAL RESPIRATION.

The plan for restoration which, up to this time, succeeds, and the only one which succeeds, is that of careful, delicate, but at the same time steady artificial respiration by the double-acting bellows. So soon as respiration and the other evidences of life have ceased, we insert the exit tube of the bellows into the nostril and empty the lung by a stroke of the bellows on the emptying side; then we gently inflate, and next empty and fill in alternate stroke. Let us proceed to demonstration. We put a large well-nourished rabbit gently to sleep with vapour of chloroform, we carry on the administration quickly, and, as you see, at the end of four minutes the animal has ceased to breathe. We may let it lie in this way a full minute, but we must not move it or handle it. The least pressure with the hand on its thorax or abdomen, or the lifting it up quickly, would be fatal to success. Now we introduce the nostril tubes of the bellows and first exhaust the chest, then fill, and in the end exhaust and fill steadily

some twenty to thirty-five times a minute. We must wait patiently for three or four minutes, keeping up the respiration, before we can expect a result; and while we wait let me narrate this peculiarly interesting circumstance: that in proportion to the time allowed to elapse between the death and the commencement of the restorative process, so must time be allowed for continuing the artificial respiration. If the animal die at once and the artificial respiration be commenced at once, one or two strokes of the bellows are often sufficient to restore independent life; but when, as in the present case, the time is long before respiration is artificially set up, then some moments must be allowed for recovery. Why is this? It is because the respiratory muscles and their nervous centres must be recharged with blood before they can take on their action again, a slow process when the motion of the heart is all but extinguished.

We persevere, and now observe we are about to be rewarded with success: if you watch closely you will perceive a tremulous muscular movement in the flank of the animal; it is followed by an inspiratory effect. The muscles are now recharged with blood, and the organic nervous centres are recharged; and this effected, I know, by experience, I have done enough. I remove the little tube from the nostril and let the recovery be completed by natural respiration. The animal will, in time, awake as if nothing had happened to it.* Was the animal in this case dead? It was practically; it was not actually. The brain was dead,

* The animal made a perfect recovery in this case. It lay without evidence of life for the full period of six minutes and a half, the longest period I have as yet known to be followed by recovery.

the cord was dead, the voluntary muscles were under no stimulus, the semi-voluntary muscles were under no stimulus; but there was one organ which, though we could not discern its motion, was in feeble action; and that organ was the heart. By our gentle manipulation we restored the heart; it recharged itself, it slowly recharged the other muscles and the nervous centres, with blood, and at last there was natural action. The experiment is so easy to perform, so uncomplicated, it would appear as if it ought always to succeed; and it would always succeed if we knew a little more. But at present we only know certain causes of failure, of which there are several. Sometimes we fail because the system is too fully charged with chloroform; sometimes because the column of blood from the right to the left side of the heart is broken; sometimes because the pulmonary vessels are contracted; sometimes because there is coalescence of the blood-corpuscles from stasis and plugging of the minute ramifications of the pulmonary artery; sometimes because the blood coagulates, and this especially if the temperature of the air is high; sometimes because, again under high temperature, the muscles become rigid; and sometimes, it may be, because the nervous centres suffer molecular change so that they fail to recharge and resume function. These seem many difficulties, but really they are closely allied, and one day we shall see a mode of overcoming them altogether. Then the results will be much more striking than they now are.

ELECTRICAL EXCITATION.

Let us again proceed to fact. We have narcotised another rabbit rapidly until it is practically dead. We could restore the animal if we pursued the same method

as in last experiment; we will use the battery instead. We will use it first with the object of restarting muscular motion in the respiratory muscles. For this object some, in the human subject, put a needle from one pole into the line of the phrenic nerve, and then apply the opposite pole to or over the diaphragm. Others put the needle under or subcutaneously upon the larynx; in plain, the position is of little moment if the second pole be in contact directly or indirectly with the diaphragm. I will bring one pole by a needle in contact with the larynx, and the other in contact with the diaphragm. And now when I make and break contact you see the respiratory act is perfect; the air enters the lungs readily, and I bring back inspiration and expiration as faithfully as if I were filling and emptying the lungs with the bellows. I continue this action, but note what soon occurs: the muscles begin to respond more feebly, and quickly cease to respond; I gently increase the current, and by this renewed action is seen again; at last the organs cease to act under any current, but if I change the current and pass it through the limbs of the animal the muscles of the limbs respond readily enough. Why not, then, the muscles of respiration? Because I have exhausted them by the electrical action. When I was sustaining respiration by the bellows my hand supplied for the time the respiratory power of the animal. I gave the force, and the muscles of the animal rested and restarted. When I used the electrical stimulus I took out of the muscles what remaining force was there—the primary force required for recovery—and under a semblance of restoring life clenched death.

But let us go one step further: this animal could not be restored now by any known method; we will then

open the thorax and look at the heart. At first sight the organ is seen at perfect rest, but in contact with the air it recommences to pulsate, and in a minute or so the auricle and ventricle on both sides are in rhythmical action. We will repeat the electrical current, letting it now pass through the heart at the lowest power. Observe what occurs: at once the organ flags and stops in ventricular systole. I have exhausted the heart as I previously exhausted the respiratory muscles.

I thought once it might be well to combine electrical excitation of the heart with artificial respiration by the hand bellows. I had no success like that which follows simple artificial respiration, and after these experiments I feel it to be unreasonable to recommend galvanic action as a means of resuscitation. Galvanism is a two-edged sword. It might, by accident I may say, in some cases, restart respiration, but it would in this respect be inferior in principle to artificial respiration, and in the majority of cases it would more effectively promote death than restore life, even when used with the specific object of exciting the respiratory muscles into natural play, or of exciting the heart. When used, as it commonly is used, merely to excite prolonged contraction of muscles, it is not aimless merely, but positively mischievous.

Still the phenomena excited by galvanic action, though as yet deceptive, are not to be ignored. One day we may see how to use electrical excitation with advantage and on a known principle; but that day has not arrived.

ANTIDOTAL MEASURES.

It has been hoped to neutralise the physiological action of chloroform by injection of antidotes into the

veins. I tried the injection of ammonia into the veins as far back as the year 1854, and although the injection excited the heart, it did not offer any material advantage. Liebreich has recently referred to strychnine as antidotal to chloroform. I had already tried that also without obtaining any indication of good result. On the whole, the antidotal method is hardly applicable to chloroform. If a man or an animal be not actually dead from chloroform, if there be still breathing and circulation, however feeble, recovery will be best aided by being allowed to occur spontaneously; if it be practically dead, the remedy is not an antidote, but a method by which the natural process of life may be correctly imitated in the motionless organism—a method carried on by force supplied from without until the inner force is revivified—artificial respiration.

The final question before us, in summary, is this: When there is a death from chloroform, what shall be done? I answer that at present we know of one process which may succeed, and that if it do not succeed we know of none other that can take its place. That process is prompt artificial respiration. But artificial respiration must be done properly to be effective; it must be done with the body at perfect rest; it must be done, as we have done it here in our experiments, by a double-acting bellows or pump which shall exhaust and fill the chest with air. It must be carried out steadily and gently; it must be withdrawn when there is any natural effort at respiration; it must be continued until a puncture of a vein shows that there is coagulation of the blood. Above all, it must be absolutely trusted until some new and definite advance is made the soundness of which is demonstrable by direct experiment. If to every hospital operating table and to

every private operating chair a double-acting pump or a bellows were permanently attached and were always ready for use, and in emergency were at once brought into action, methodically and with fixed purpose, under the surety that if it failed all means would fail; if the body were kept, meanwhile, in entire repose, so that no violence nor shock was allowed to check the little remaining power of the heart, then I believe, from experimental data, that at least one in every three or four of the cases now ending fatally under chloroform would be saved.

COMMENTARY, 1892.

I wish I could add, in concluding this course of lectures, any new mode of resuscitation of ready and effective action. I cannot; but two or three advances have been secured, and deserve to be named as leading to good and important results.

INVERSION OF THE BODY.

In some of my earliest experiments I noticed that when the heart of an animal dead under chloroform was exposed, and had ceased to contract, contraction of the right auricle and ventricle could be re-established by the simple process of inverting the body. Under inversion the blood flowed from the inferior cava into the auricle, and thereupon active movements took place both in the auricle and the ventricle, with the effect of filling the lungs with blood through the pulmonary artery. I tried then, by artificial respiration, to re-establish a current from the right to the left side of the heart, and repeated the experiment many times. I found, however, that the success of artificial respiration was as well marked when the body was simply on the incline, with the head downwards, as when it was

actually inverted; and so I did not press the matter. Later M. Nelaton, in a case of chloroform collapse in the human subject, seemed to have restored life by inverting the body; and quite recently, by combining artificial respiration with the inverted position of the body, Dr. A. E. Prince, of Illinois, produced a remarkable resuscitation. He suspended the body of a collapsed patient on his back by bringing the legs of the patient over the shoulders of his assistant, who then moved about with the apparently lifeless body, while Dr. Prince inflated the lungs by direct insufflation. The result was most satisfactory. The diaphragm was raised by the filling of the thorax, and when the tension was removed it emptied the chest by its descent and by the pressure of the viscera resting upon it; in time the natural respiration recommenced, and the patient was restored to life. This was a brilliant success, but it must not raise too great expectations. It was an instance in which the column of blood from the right to the left side of the heart, never completely broken, was quickened by the movement into sufficient vitality to allow of recovery. In examples of collapse in the first stage, where syncopal apnœa is the mode of death, this method is most promising; and it strongly suggests the introduction of an operating table which will admit of the see-saw movement described in the last ASCLEPIAD.

INJECTION OF PEROXIDE OF HYDROGEN SOLUTION INTO THE PERITONEAL CAVITY.

Some of the most extraordinary recoveries from death collapse induced by chloroform occurred to me from another line of research. I thought that if oxygen were liberated into the peritoneal sac, there would be

set up two revitalising actions. There would be the direct vivifying action of the oxygen on the diaphragm, and there would be direct absorption of oxygen into the blood, with the revival of contraction of the right auricle and ventricle. The experiment was of the readiest and simplest kind. Into the peritoneal cavity of animals to all intents dead from chloroform—so dead that the respiratory movements were nil, the heart movements perfectly inaudible, and the limbs entirely flaccid—half an ounce of the solution of hydrogen peroxide of ten-volume strength and mixed with three times the amount of water, the whole warmed to the natural temperature of the animal itself, was injected by a hollow needle into the peritoneal sac. The effect was the almost immediate return of respiration by the restoration of the diaphragm into movement with a distinct renewal of cardiac motion. The phenomena induced were, in fact, those of rapid resuscitation; but they were checked before the return of conscious life by cessation of the circulation and fatal apnœa. The cause of this diversion of symptoms was obvious enough when the bodies of the animals were inspected. The oxygen had entered into the venous system, changing all the venous blood into arterial, with separation of oxygen in the free state in the form of bubbles which would not pass through the capillaries of the lungs. Death consequently took place in the same way as when air is injected into a vein.

THE DUALITY OF THE MIND.

IF those who have never studied the subject of the Duality of the Mind are not astounded with its importance when they arrive at the close of this short essay, I shall feel sure that a fault of my own has been committed; that I have failed, in short, in making a momentous study clear in respect to its quality. The duality of the mind is a subject of commonest every-day conversation, and yet it eludes observation. It is an open question in a sealed book. The most frequent forms of speech reveal our general knowledge of the duality, and yet we treat mind as if it were always unity—unity in thought, in action, in expression.

In my present effort I shall be led to consider the qualities of the mind as dependent upon certain well-defined anatomies of that system of the body we call the nervous system. In the lower forms of life we discover a cord or line of nervous beads or ganglia, from which emanate a series of nervous lines or nerves to all the moving structures. When these lines of nervous cord, with their expanses, exist alone, we have animals that live and move almost altogether automatically. They make progression after what may be called a stereotyped fashion; they exhibit what have been named as instincts, modes of movement, by which they

seek for the food that sustains them; they have organs of sense by which they are rendered conscious of obstacles in their way, and from which they move; but all through the actions they perform are mechanical, and not under the direction or the influence of reason. In this system also the motions called the passions have their centres; for the passions also are automatic.

When we ascend into the higher scale of being, we find, in the highest type of being, man, a much more elaborate arrangement. We find two definite nervous systems—one ganglionic, in lines of nervous structure, with connecting ends, or nerves, supplying all the organic parts, and ministering to the involuntary or vegetative life; the other, much more complicated, a system in itself for the purposes of volition, a system which is the seat of the mind, a system which, through the senses of taste, touch, smell, sight, hearing, receives the vibrations of the external universe, treasures up those vibrations in concrete form and order, and gives them out in action, inspired by what we call will or voluntary life.

It is with this last-named system we have now to deal. In it lies the duality; it is the centre of the dual faculty, or faculties; it is constituted for a dual action, its anatomy tells its duplicate function. In the ganglionic system all is unity; it is a mechanism automatic in its work, steadily charged up and steadily giving up its force as it is charged; a continuous energy in waking hours and sleeping hours alike. Certain acts must be done, whether we will or not, that life may be continuous. The heart must beat, the intestines must propel, the glands must secrete and excrete, the arteries must be controlled or regulated, and by and through the involuntary centres of life all this work is done.

But what we will ourselves to do is done by its own system, called the cerebro-spinal. Is this sytems single or dual?

Hitherto the world has been led into what may be called the metaphysical method of reasoning on this singular inquiry, and, in consequence of this mode, we have learned to look upon the mind as dependent on one single organ, the brain. Few persons are professed metaphysicians, and metaphysical, as special studies, are gradually breaking up in the presence of physical. It is impossible, however, to describe how intimately many old metaphysical lessons have been engrafted into our thoughts, and have become beliefs. This is one. The metaphysical idea of mind is of unity with variety of phenomena, acting, sometimes, as if it were a double mind. Consciousness and common sensibility may be considered as having one seat mentally, yet they may be separated; there may be consciousness with a loss of common sensation or sensibility. A man conscious of all around him, as it would seem to the looker-on, may be unconscious of his own existence, and may be subjected to what would be torture, were he conscious, without being aware of any injury inflicted on him. The mind, again, may be considered as one, and yet it may be open to contention or difference, as if it were seated in an organ that had double attributes. We find in all kinds of philosophies, in all kinds of theosophies, this contentious spirit; and I could fill many pages with teachings all leading to the same kind of belief.

Origin of the Dual Theory.

It is not until we come to the first half of this century that we arrive on a solid basis of knowledge, founded on direct observation of the meaning of these

phenomena. The late Sir Henry Holland, in a chapter on the brain as a double organ, in his *Medical Notes and Reflections* was the first, perhaps of all, to touch the border of the truth. He, in this essay, threw a light into an obscurity which he did not fully illuminate. Fortunately he was quickly followed by another keener and bolder explorer, who, in 1844, announced the theory of the "Duality of the Mind" on grounds so sound that he may be said to have made an actual and astounding discovery in natural science. This discoverer, of whom probably very very few heard, was the late Dr. Arthur Ladbroke Wigan, a man whose whole professional life was devoted to the study of the mind in connection with the body. Wigan was a good physiologist, a good anatomist, and an able physician. He was a man well acquainted with the leaders of his time, and was well advanced in interpretation of character. He had known the great surgeon Cline remove a peculiar form of insanity by lifting from the brain of an insane person a portion of bone that pressed on one particular part of that organ; and he was most assiduous in comparing diseased conditions of brain found after death with the phenomena of mental activity or inertia that had been presented during life on those from whom the brain had been removed.

It seems to me, from a careful examination of Wigan's treatise, that he was not so learned as we are now on the unity of action of that ganglionic or involuntary system of nerves to which attention has been already drawn. Had he known all we now know, his theory would have been much more strongly supported than it was when he propounded it. Perchance, too, in his desire not to be forestalled in his discovery, he rather hurried the publication of his great work and great

argument. Be that as it may, he laid a foundation which is not likely ever to be permanently destroyed.

We have to turn to the grand voluntary nervous system, the cerebro-spinal system, in order to get at his meaning in its simple interpretation. All the external nervous expanses, whether commencing on the surface of the skin, or on the surface of internal membranes, or in organs of sense like the eye, the nose, the ear, the tongue, pass, in collected lines of nervous cords, to the common centre, the brain. Some, like the nerves of the organs of special sense, pass direct to the brain, others pass through the spinal cord, and, by decussation, make their way to the opposite sides on the brain, those from the left half of the body passing to the right, those from the right to the left brain; but all meet in the brain centre, the "metropolis of humidity," as Hippocrates called it. To this extent, to the extent of nerves from many parts converging to a common spot, there is unity in the cerebro-spinal system. The duality commences in the brain centre. There are two double brains—the two cerebrums, the two cerebellums. The cerebellum, or smaller brain, is dual, like the cerebrum, or larger brain, but is a motor co-ordinating centre, not a mental. The true organs of the mind, the centres, in which the faculty of thinking and of willing takes place, are the cerebrums.

The Cerebrum as a Dual Organ.

The cerebrum is dual; it is made up of two hemispheres; so the anatomists have taught up to the time of Wigan. Here he breaks in, and propounds the new theory. "The term 'two hemispheres' is," he says, "strictly a misnomer"; the two together form very

little more than one-half of a sphere, and they are really and in fact two distinct and entire organs, each respectively as complete and as fully perfect in all its parts, for the purpose it is intended to perform, as are the two eyes. Each cerebrum is a distinct and perfect whole as an organ of thought. A separate and distinct process of thinking may be carried on in each cerebrum; each cerebrum is capable of a distinct and separate volition, and of opposing volitions.

Such, in a few words, is the starting-point of this bold and original theory as set forth by its author, and on this he expands into many remarkable propositions.

In healthy persons one cerebrum, or brain, is almost always superior in power to the other, and is capable of exercising control over the volitions of its fellow, and of preventing them from passing into acts, that admit of being manifested to others. In persons in whom one cerebrum has become the seat of functional disorder or of positive change of function of such a kind as to vitiate the mind or induce insanity, the remaining healthy cerebrum can still, up to a certain point, control the morbid volitions of its fellow. In this manner he conceives some are enabled to "*self-govern*" insane tendencies; but when disease of one brain becomes sufficiently aggravated to defy the control of the other, there is then exhibited one of the most common forms of mental derangement; while a lesser degree of discrepancy between the functions of the two brains constitutes a state of doubtful insanity or conscious delirium. In conditions with one brain diseased and the other healthy there may be lucid intervals, but with both brains diseased permanently there is no lucid interval. Insanity is the permanent mental state.

I have condensed in the above the essence of the theory of the duality of the mind. It is so simple, that he who runs may read it. I may say at once that accepted, on sound inquiry, it does more to explain the mental organisation and function than any other that has ever been placed before the world. The importance of the theory is, however, the best of all reasons why it should not be too easily accepted. It should be tested in the most rigorous possible manner. Let us put it fairly to the test.

It may be objected to the theory that the two brains, although they are distinct in form, are made one by the interlacement of one brain with the other through the connecting bands or commissures, and especially through the great commissure, the corpus callosum. Wigan himself meets this objection with entire correctness. The corpus callosum is not an interlacement of the mass of the hemispheres; the raphé defines the distinction. The corpus exists as a floor holding the hemispheres together without connecting them. The anterior, middle, and posterior commissures, if they be purely commissural, are so to minor parts alone; the great antero-posterior commissure acts for communication from one part to another of its own sphere. It is a longitudinal as distinct from a lateral form of communication. Anatomically, there is no direct intro-cerebral communication; if there were, why two brains? If it be argued there are two, that one may act for the other in case of disease or accident, then is it the more certain that the two are distinct organs, either of which can act independently,—which is, in fact, the whole argument.

If the two brains were one, acting as one in the healthy man, there must be perfect unity of structure,

unity of capacity, unity of detail in every part, in order to sustain unity of function. If the two brains vary in details of form there must be dual action. The argument of Wigan is that the two brains do vary in details of form, and since his time many facts have been recognised, which strengthen his thesis and bring it nearer to proof.

The evidence is positive that one cerebrum can act and maintain mental activity without the aid of the other. The results of disease, the results of experiment, afford this required proof. Cruveilihier has reported an example in which a man, forty-two years of age, who up to his death enjoyed the use of all his senses, possessed the ordinary degree of intelligence, and walked, by the help of a stick, died of heart disease two days after admission into hospital. His intellectual faculties appeared to be entire. In this man, nevertheless, it was found after death that the left brain was three-parts destroyed and replaced by watery fluid. The mental acts had long been carried on by the right brain. This is one only of many other examples. I could supply another from my own experience. It is unnecessary; it is certain that one cerebrum is sufficient to maintain mental activity, and so much granted, is it probable that each cerebrum (itself an independent organ) is so uniform in construction with its fellow-brain, that it acts with it always synchronously, obeying the same impulses and sending forth the same volitions?

It is not probable; it is not possible.

The two brains vary much in weight, in consistency, in form. They carry different volumes of blood; they are fed by different arterial streams: they must, therefore, have nutritions differing according to the circulation

that plays through them. There is, it is true, a certain common circulation of the return or venous blood in the sinuses by which the whole volume of blood in the cranial cavity is generally equalised; and in the cerebro-spinal canal there is a common space for the cerebro-spinal fluid; but these are not sufficient to reduce the two grand organs to a common unity. They balance the circulations, but they do not bring all into one; they do not interfere with the independent action of each brain; each brain receives and returns its own blood; each brain works, and, if I may express myself in words true though plain, each works, and excretes, or perspires, according to the amount of work it executes.

The left brain, or that which supplies and animates the right side of the body, is the most active brain, as a general rule. Dr. Wigan propounded this view, and he was right in doing so. But since his death we have discovered a striking piece of new evidence in favour of his general theory. We have localised the seat of language in the left brain. We now know that with diseased conditions of one portion of the left brain there is developed not simply paralysis of the right side of the body, but loss of speech—aphasia. To a large extent this fact alone declares the individuality of one brain, the left, and crowns the demonstration. No experiment that any man could devise would add to the completeness of this experiment produced by nature in the course of disease.

Taking the evidence as a whole, it appears to me that every point of it directs us to accept the duality of the mind as based upon the anatomical fact, that we have two brains, as in the examples of two eyes, two ears, two hands, and as in other examples where two organs are required. When one organ alone is requisite there

is one, and no more: one stomach, one heart, one liver; and one united nervous or ganglionic system, and no more.

Illustrations of the Dual Theory.

Up to the present point I have kept to what may be considered the material study of the dual theory. It is time now to inquire how far the said theory chimes in with the details of the mental phenomena we see ever going on around us. We talk often of the universe that environs us. We hesitate as we think of the vastness of that universe. How can we, merest mites of mites, read that seen and unseen space in which we float? We could not think of it with one brain; with one brain we should resolve into the immensity. We must have a grip of mind on the earth as we roll in space. There must be in our microcosm two living worlds with their homes or centres. Wigan expresses this by saying "that every man is, in his own person, conscious of two volitions, and very often of conflicting volitions, quite distinct from the government of the passions by the intellect; a consciousness so universal that it enters into all figurative language on the moral feelings and sentiments, and has been enlisted in the service of every religion."

The statement is most true, and explains in rational exposition many seemingly contradictory problems relating to individual lives and events.

I had the opportunity once of meeting the late Mrs. Booth at the house of a mutual friend. We discussed, as a matter of course, the work and results of the work of the Salvation Army. The topic to me was new, and found in me little of quickening sympathy, But I was in the presence of an enthusiastic, earnest,

and able woman, and some things she said struck me forcibly. We were speaking of what is called conversion. "It is a phenomenon," she observed to me, "that you, even as a physiologist, ought to study. There is a man, steeped in sin: he is brutal to wife, children, parents; all domestic ties are as nothing to him; he is submerged in iniquity; you, I make bold to say, could do nothing with that man; you might as well attempt to raise a huge sunken rock out of the sea as lift him out of his lost position. But we succeed in reaching that man; we preach to him, reason with him, pray with him, and practically he becomes a changed being; foulness of speech ceases to fall from his lips; the home cruelty is to him a horror; he is as a little child, and as if he were literally born again."

"And after a time he relapses," I suggested.

"Sometimes, not always, and, as a rule, the very worst cases relapse least frequently; some never relapse; and if but one of this kind on being converted does not relapse, but progresses in what we call a reclaimed life, that one case alone is, or ought to be, a study for you and all men of science. We will not argue on the question of cause now, but I want to impress on your mind the facts, because it is on such facts, often repeated, that we stand so securely."

I admitted the facts, but we had not the time, if we had the inclination, to discuss causes. Mrs. Booth, I doubt not, had her explanation at hand; I have mine, and mine is founded on the theory of the duality of the human mind. Her model submerged man appears before me as one governed for long years by an evil brain. So long as that evil brain retained its dominant strength it ruled the man. But there came a time

when that excited brain wore out into feebleness, when impressions upon it derived from the second brain began to act with superior force; when doubt and contrition thereupon agitated the man; when he felt that he had in him two volitions beyond his mere animal instincts and passions. At this crisis a strong and earnest external nature fell upon him, roused into action his own better nature, drove his lower nature into obedience of fear, and, temporarily or permanently, transformed him into that which he had never yet experienced—into a man in full exercise of a newly developed strength. That man, physically and literally, was born again. We need not criticise the means employed for that regeneration; we will not, at this moment, question whether the training that followed the new birth was the best and only best; but we must admit the phenomenon of the change. There was about it no mystery; it was, in scientific definition, an organic mental transformation; the awakening into life and living action of an organ in a state of partial inertia; a physical conversion leading to new action, and, if we like to say so, making a new man. No wonder, from this reading, that the worst specimens of vice should become, under the change, the most lasting specimens of virtue.

I have related here a sharp and striking example of change of power, or dominance, from one to another centre of the mind. But there are endless other illustrations quite as convincing. A youth and young man struggles for many years of early life in adversity. He lives according to his environments, and with one brain attuned to these he acts from them. Suddenly a better and latent brain becomes roused into action, and steadily the man rises in fame, becomes, perchance, a

marvel of his generation. The now dominant brain carries all before it, until at last, under the wearing excitement, it ceases to burn with its usual light. It pales, it wearies, it depends on its lower helpmate, and gradually the friends of the man observe how he has changed. He falls back on his old narrowness; he fears poverty while he is rich; is intent on making money; harasses himself about domestic troubles; loses his exalted powers of memory, imagination, analysis, construction; is restless as to place; is filled with internal contentions; thinks the world inappreciative of his genius if the world is not at his feet, and when it is there feels how much all is vanity and vexation of spirit. Then suddenly he goes out, from absolute failure of the once greater brain, and, like a star in the firmament, rapidly extinguished, is seen no more. His ashes remain to be worked up into new forms for human apprehension, while the vibrations he has sent forth during the progress of his mortal life remain in motion for evermore, for the delight or instruction of generations unborn; creations of his brain that may outlive for ages their projector.

Duality of the mind in its marked character is ever before us; there is not a single work of imagination pourtraying human character in which the duality is not exemplified. Fictionist and poet live upon it; and I notice recently that one of the most analytical and realistic writers of fiction in France, and one of the most popular, M. Guyot, recognises not only the art but the science of the method. In describing one of his imaginary characters, or more probably a real character transformed from the ordinary daily life into his pages under a false name, he explains duality on the same principle that Wigan did long before him. He is dealing

with a man who has two brains—brains which are not of the same mould, and, therefore, not of the same action—and he writes his character from this standpoint with all the vigour of a master in literature and the precision of a man of science. Guyot is alone, as yet, amongst writers, in that he knows the physiological basis of his argument, and is one of a thousand who has used the argument with a knowledge of the foundation of it in material form. The writer of the wonderful Book of Wisdom knew the fact of duality perfectly. James the Apostle defined it: "A double-minded man is unstable in all his ways." Every poet, every playwright has known it; and our own national poet knew the duality so well, he never pourtrayed a character without it. Hamlet is confessedly to himself a dual nature, and the actors who have represented him most faithfully have been they who have most keenly dissected out the two parts. Macbeth has a dual mind always in action, and shows the fact from beginning to end; while his wife, with one brain dominant to the end, stands out as if of single mind, until the overwrought organ gives way altogether, and allows a worthless, timorous, foolish brain to come into play. But the perfection of knowledge of duality by Shakespeare is in the rendering of Richard the Third in the night scene on Bosworth field. At dead midnight the king, in his tent, arises from sleep; "Cold fearful drops hang on his trembling flesh," as he questions himself in his dual nature, "What do I fear? myself! there's none else by: Richard loves Richard"; "that is, I am I. Is there a murderer here? No;—yes, I am: then fly, —what? from myself? Great reason: why?—Lest I revenge. What, myself upon myself? I love myself. Wherefore? for any good that I myself have done

unto myself? O no, alas, I rather hate myself for hateful deeds committed by myself. I am a villain; yet I lie, I am not. Fool, of thyself speak well:—fool, do not flatter. My conscience hath a thousand several tongues, and every tongue brings in a several tale, and every tale condemns me for a villain." And so the colloquy progresses into despair. "I shall despair. There is no creature loves me; and if I die, no one will pity me:—nay; wherefore should they? Since that I myself find in myself no pity for myself."

A late friend of mine, a fine dramatic critic of the old school, who knew Edmund Kean, and who retained the gift of a walking-stick of that famous actor, told me that in this memorable scene Kean so impersonated two minds that he seemed to bring two men actually on the stage contending with each other, and yet filling always the same place. "The actor so changed expression that the character doubled before the eyes of the audience, leaving it spellbound, and as if every one had forgotten to breathe."

Wigan was of opinion that no single person has the two brains exactly balanced. He considered the most powerful mind was that in which the two brains, both being healthy, were nearest to each other in capacity; but he believed that that was also a good condition in which one brain took the leading part, and in which the other brain, in accord with the stronger, lent the stronger its assistance. A mind sustained in this manner was one strong for endurance, and one that was able to undertake, with certainty of successful work, the most difficult and hazardous labours. The minds of men like Cromwell, Washington, Wellington, Franklin, would be of this last-named cast, while the minds of the giants of intellect, of Napoleon, Newton, Columbus,

would be constructed on two brains of equal power, both in unison in their capacity and will for action.

In common life the two brains act together with sufficient unity to maintain the mind in a fairly balanced state, but there are conditions in which the divergence is so considerable that balance is broken, and then there is what we call insanity. Wigan sums up the argument in a very clear manner. He says: "I think it may be assumed, without risk of contradiction, that the fact of each brain being perfect, and a complete instrument of thought, is abundantly proved. That each, while in health, corresponds in action with its fellow, is obvious from the fact that this unison and correspondence give only one result, as in the case of two eyes producing single vision. That when, from any cause, one brain is disordered, a discrepancy in the two processes of thinking takes place. That the healthy brain (aided by the action of such of the organs of its fellow as are not affected by the disorder which disturbs the others) can, in nearly nine hundred and ninety-nine cases in a thousand, according to the usual proportion in this country, control all manifestations of morbid emotion or judgment, but that the thousandth case is the madman."

The balance is sufficiently preserved on the grand scale to prevent the intrusion into the world of an excess of insanity. At the same time there is a wide range of diversion, short of obvious destruction of balance. Heredity here plays a leading part.

Effects of Heredity.

It must do so. In the course of heredity particular forms or moulds of bodily type are constructed in line. Features external in character typify the family stock;

features internal in character typify the family stock. We see one set visibly in form; we see another set visibly in action. Why not? The two brains are cast on the hereditary plan, and follow the laws of heredity. Give, therefore, a type of irregularly formed cerebra in a family, and almost of necessity there will be some continuation of family proclivity. Towards this four kinds of construction unite: that of the enveloping bone or skull; that of the arterial vessels with which the cerebral masses are supplied with blood; that of the material, out of which the brain substance is formed as a solid organ; and lastly, that of the cellular development, which is, I believe, of all the most afflicted by heredity.

In the case of other double organs than the cerebra, one organ is capable, under emergency, of replacing the other; that is to say, of doing the work which the other cannot accomplish. This occurs in regard to the kidneys, the lungs, the eye, the ear; and when it happens that the officiating organs are free of compression, so that they can grow, to use an ordinary term, to larger size under increased supplementary work done under pressure, one may become, in time, equivalent, in respect to work it can do, to two. But when the organs are always working under fixed compression, as in the skull, compensation of the kind named is not in the same way possible. It is true that an animal can live with one brain alone in action, and it is equally true that an animal can live with the larger part of both brains destroyed; but it is not compensation that is presented under such circumstances. The life is maintained with the intelligence lessened to the extent of destruction. The brain organs, that is to say, are not constructed for one of them to act unceasingly

for the other. When one brain alone is injured, the other does no more than continue its own work, and the mind retains its lucidity according to the natural working of the part that is uninjured. In plain terms, the independency of the two organs is an inevitable fact. If they work in unison, from the circumstance that they are similar in build, all the better; but the independency of the working is absolute.

I accept on my part the duality of the mind, and the independency of two brain organs as a demonstration. Everything we can put into test regarding the theory comes out clearly; and I doubt if there be any theory in physiology that bears so close an inspection.

Practical Results.

It may be asked what practical results are derivable from this theory? Is it a mere curiosity—is it a grand exposition? It comes before me as one of the grandest expositions ever revealed in the study of mental science; and I will, in conclusion, indicate a few, very few, natural secrets it seems to me to unfold.

In the first place, the dual theory gives the best explanation of certain forms of insanity, and specially of those forms of insanity which are hereditary in their character. Some insanities spring from direct external influences, due to substances taken into or produced within the human organism. These are probably effective on both brains, and the effects may be considered as deliriums or intoxications, like those produced under first intoxications from alcohol, the sulphur compounds like mercaptan, or the vapours of chloroform, ether, or anylene. They are temporary casualties at first, and when the two brains are naturally balanced and healthy

they do not fail to be removable under judicious treatment. But the true and most lasting forms of insanity are those where, from the parental stock or type, the two brains are out of tune by natural or unnatural construction. Under such conditions one brain, suffering from exhaustion or excitement, becomes out of accord with the other; two minds then exist in the same organism, and in the difficulty the want of balance is declared in the irregular and delusive thoughts, expressions, and deeds of those affected. Again, in some, in whom heredity of construction plays no important part, irregularity of development of brain or of work produces irregularity of structure for the accomplishment of mental labour. One brain becomes too excitable or too powerful for steady balance. Its possessor is charged with one series of ideas; and these, unchecked, run wildly into conflict with the commoner brain work, and, gaining supremacy, produce the imperfect equilibrium which indicates the mind diseased. The two brain centres unbalanced, mental aberration is an easy sequence; as is another condition leading to aberration through the passions. The passions which so influence mankind are centred in that involuntary nervous system which we call the ganglionic. They are spontaneous acts, and would be always in motion were they not governed by the reasoning centres, the two cerebra. When, then, there is discord in the reasoning centres, the passions from their seat have free play, and emotions of ungovernable kind lead to animal propensities and acts which of themselves are often raised into the worst so-called insanities. Or, when the two reasoning centres are both of low character, then again the passions prevail. Or, once more, when the governing centres of strong character become enfeebled from

overstrain or prolonged labour, the same loosening of passions long held in abeyance takes place. In this way it sometimes happens that men, who for many years have been the powers and lights and models of the circle in which they move, drop, in their early senility, into animal sensuality, and occasionally themselves defame a name they might have left in the brightness of fame. The effects of degenerations, from the opium habit, alcoholic inebriety, and other depressing influences in earlier life, lead also to reduction of controlling power of one or both of the cerebral or brain centres.

As we gain a physical insight into these modes of motion belonging to our common humanity, a world of light breaks upon our view, teaching wisdom as well as knowledge, and charity as the outcome of both. Our hard-and-fast and narrow dogmas pale in this light. We see before us the healthy, the diseased, the criminal minds at their work. We dissect out the principles leading to motives to detect that motives, good and bad, have their constructive causes, and that a person actuated by good or bad motives must be constituted, temporarily or permanently, for the one or for the other. Some day in the future our criminal codes will have to be reformed altogether, on a system evolutionised from a knowledge of this physical basis of good and evil.

The grand lessons, the immediate lessons, derivable from the study of the duality of the mind, and of the passions as automatic wills distinct from the mind, the grand lessons have reference to the education of communities, first, into structural form and strength; and, secondly, into cerebral activity with uniformity or equality of power. When we see the effects of debility and deformity of the framework of the body in modifying

the equality and power of the central cerebral organs, we learn that with physical impoverishment there can be no intellectual advancement, no control over the animal emotions and passions, no great types of intellectual capacity, no true and sane balance of mental powers. In plain words, we see that to secure sane balance we must build up the mansions of the mind with artistic skill before we attempt to furnish them.

When we have built we must furnish with mental attributes of the best order, for the best purposes; never oppressing one brain at the expense of the other, or the two as one. Dr. Wigan knew the famous statesman William Pitt, whose reckless father, the Earl of Chatham, forced him to unnatural labour, when, as a lad, he was overburdened with the variety of topics, by the saying, "Courage, my son: remember there is only the cyclopædia to learn." He saw the great statesman at Bellamy's, when, in a state of collapse, after the excitement of debate, with a countenance bearing the air of insanity, he would swallow a steak almost without mastication, drink a bottle of port wine almost at a draught, and then, barely wound up to the level of ordinary impulse, repeat the process twice or even three times in the course of the night; a frightful example of over-cultivation of brain before it had attained its full development, and so exhausted by premature and excessive moral stimuli, that when its ambition was sated, it was incapable of even keeping itself in action without the physical stimulants spoken of. Men called the sad exhibition the triumph of mind over matter; Wigan called it the contest of brain and body with victory obtained at the sacrifice of life. To which he added "that it is not to compare little things with large to speak of similar examples in humble

life, for the mind of the humblest mechanic is as important in the scale of creation as that of a king; and I have seen," he continues, "the same wretched results in the case of a little boy perched at a desk as the accountant in the shop of a draper, who had to attend, in rapid succession, to forty or fifty young men bringing their notes of sales to him to be verified and registered, to give correct change to every one while his attention was solicited by half a dozen at a time; and who, rendered responsible for the slightest error on pain of dismissal, went to bed in a state of exhaustion worse to endure than the most excessive bodily fatigue."

In such examples, training of the two mental centres into uniformity of action is simply impossible; and yet in this day, fifty years after the above was written, the same primary errors go on despite our enlightenment, and, as might be foreseen, with increasing insanity; broken balance of mind on every side, a second Babel the ultimate destiny.

I close here, leaving many other lessons that could easily be drawn, for inference rather than description. Suffice it to sum up with these facts. (1) That all mankind is dual in mind by natural construction, so that a congregation of human beings, large or small, a family circle, a private meeting, a parliament, a nation, must always be reckoned as twice its individual number before its mental constitution and strength can be properly appraised. (2) That the efforts of all should be directed to the proper construction of the casket of the mind and the physical powers working it. (3) That mental work should be for progress in ways of unity of purpose, towards greatness of life and character.

*ON INTRA-THORACIC AUSCULTATION: A NEW DEPARTURE IN PHYSICAL DIAGNOSIS.**

MR. PRESIDENT AND GENTLEMEN,—I had promised to bring before the members of the Medical Society of London this session a paper on "Some New Researches in Synthetic Pathology," but as I was not prepared for so early a call for the paper as that which has been assigned to me, I have been unable to complete it in the form in which I would wish to present it. It happens, however, that I have at hand a short communication which I have much pleasure in submitting to you, and which, because it is extremely simple and practical, will, I trust, be accepted as a substitute for the longer and more ambitious essay.

I call the present essay a study of Intra-thoracic Auscultation, a New Departure in Physical Diagnosis, and I cannot introduce it to your notice better than by relating how it came into my mind and practice. A few months ago a patient consulted me who was suffering from serious and obscure symptoms referable, by the process of exclusion in diagnosis, to the upper portion of the alimentary canal. He had lost flesh

* Paper read before the Medical Society of London on October 31st, 1892.

to an extreme degree, was very feeble, had often a difficulty in swallowing food, at times retained food of a fluid or semi-fluid kind in the stomach for long periods, and then after suffering severe pain vomited it with difficulty, returning it in a partially digested state. I looked upon the symptoms with suspicion as possibly indicating malignant disease of the lower part of the œsophagus; but as I found he had been following an imprudent dietary, I was content at the moment to regulate diet carefully and to prescribe a mixture of dilute hydrochloric acid and pepsine. He left me, to return in two months reporting himself in every respect better. He had gained in flesh, he retained food, was free from acute pain, had improved in strength, and had lost a sense of weariness of mind as well as body, which had been most oppressive. He had determined to take a holiday, and I agreed with him that the change he suggested would be advantageous. I did not see this patient again for three months, when he consulted me once more in consequence of a sudden return of his worst symptoms, to which were added others pointing more decisively to œsophageal mischief low down in the tube. With difficulty he had partaken of a rather too copious meal one day previously, and soon afterwards had been seized with acute pain, which lasted until the undigested mass had been vomited, with free secretion of the gum-like mucus characteristic of stricture. He was again greatly emaciated, presented a condition of circulation so feeble that I could scarcely detect the radial pulse, and the heart was so weak that it was difficult to distinguish clearly the two sounds.

I tried in this case what I have called the water-gurgle test for the diagnosis of stricture, as described

in the ASCLEPIAD, vol. vii., p. 332; that is to say, I got the patient to attempt to swallow fluid whilst I auscultated in the line of the œsophagus anteriorly and posteriorly. Whenever there is true stricture of the organic class I have usually found by this method a point where there is heard a loud gurgling sound on attempts to swallow, followed by a sharp noise as of a passing current of fluid through a constricted passage, and I think there are few more characteristic points of diagnosis of stricture existing in the lower third of the tube. There was no response to this test in the present instance, and the patient expressed to me that the tumbler of milk-and-water which he had swallowed went down without his being conscious this time of obstruction. I turned, therefore, naturally to the use of the œsophageal tube—an operation which led me to the new facts I have to record. I passed along the œsophagus a medium-sized tube and ran it without difficulty down to the stomach. There was no serious obstruction at any part, but I thought I experienced some sense of friction of a very slight kind. Whilst endeavouring to be certain on this matter an idea which I had once before had in my mind, but had not before acted upon, suddenly occurred to me. Why not auscultate through the exploring tube? At once I sliced off a portion of the free end of the tube obliquely, slipped over this sliced end the terminal part of the double stethoscope, and made in this fashion the exploring tube a continuous stethoscope. The effect of auscultating in this way was most interesting and satisfactory. I could hear soft friction of the tube against the walls of the œsophagus, and was made quite sure that the friction was uniform throughout and that there was no special constriction or induration in any

portion of the tube. When I passed the tube into the cavity of the stomach itself I obtained a sound new to me, like a gentle seething as of air or gas agitated in a thickish fluid, and at times a gurgling sound of gas, with another sound probably due to muscular contraction of the stomach itself. As the patient experienced no trouble or inconvenience during examination, I had ample time for inquiry; and I leisurely withdrew the tube, noting the sounds audible in the course of the movement. In the tube at this time there were only two openings, and those at the extreme end. I succeeded therefore in catching sounds at such points only as were in apposition to the openings. I withdrew the tube until the opening on the left side came in contact with that portion of the œsophagus that lies in immediate proximity with the heart. By previous auscultation of the heart over the thoracic wall I had failed to detect clearly the two cardiac sounds owing to the feebleness of the cardiac action, but now both sounds were as distinct as they would have been from a normal heart. They were not, however, the same precisely as the sounds we hear through the thoracic wall: they were duller in character, as if they wanted the resonance which is probably produced by the pleura stretched over the thoracic cavity. At the same time they were loud and were singularly distinct. By moving the tube gently up and down I could get the second sound separately from the first, and *vice versâ*; but when I had the opening of the tube midway so as to compass both sounds there was not so much difference between the first and second sounds as is distinguishable under ordinary auscultation. I was quite prepared for all these modifications of phenomena; they corresponded precisely with what I had learned

many years ago when, in combination with the late Drs. Baly and Sibson, I had seen Dr. Halford demonstrate Brien's valvular theory of the cause of the two sounds. We listened at that time directly to the sounds from an opening in the chest wall of a lower animal under anæsthesia, and detected that with such immediate auscultation the sounds were deficient in sharp resonance, and were more equable in tone than was common from ordinary auscultation. It was the same now. I counted the beats of the heart very deliberately from the inside of the thorax, seventy beats per minute, the sounds and the pause in proper order and the action perfectly regular. I expected that on withdrawing the tube further out of the œsophagus it would be possible to hear a loud sibillant or vesicular murmur in respiration. In this I was disappointed to a certain extent. It was impossible to catch a murmur, even on a deep inspiration, so distinct as the murmur heard from the chest wall outside.

From these observations I have been led to the new departure in physical diagnosis in which I am anxious others should take part, and I have devoted some time to certain preliminary steps in its development. Briefly it is a means for auscultating on an extensive scale the organs of the body *from within the body*. I shall occupy most usefully the short remaining time at my command by indicating, first, the lines of research in which the plan promises to be most useful; secondly, the limitations of the plan and, if I may so express myself, the objections to it; and thirdly, the modes by which it may be improved from this its original start, so as to make it ready, safe, and in its broadest sense useful.

Touching the first of these points, the method promises to be useful—I hope very useful—in the

diagnosis for which I first applied it. It proved of service to me; at the moment it told me distinctly, in the case I have referred to, that there was no constriction, no induration, at any part of the œsophagus, so that the hearing confirmed the touch, or, rather, corrected it in a manner that could not have been more satisfactory. The advantage will be that stricture may be detected in its very earliest stage—the stage in which, according to my experience (unfortunately an experience specially large in this disease), there is the only chance of doing good by dilatation. The new auscultation may prove also an aid to diagnosis in diseased conditions of the stomach itself. It is certain from the sound I heard from within the cavity of the stomach that there are going on there changes of a physico-chemical kind, leading to a sort of effervescence that is distinctive in character. It is most probable there are differences of sounds connected with special fermentations, and that by study and experience these differences may become detectable at once by auscultation. Here there is an open field for research into which I have been unable as yet to enter. Again, as bearing on the stomach, the motions of that organ evidently produce sound that will admit of interpretation. A healthy stomach may possibly give forth no sound, or it may give forth a definite sound when full of food, with other sounds during various stages of digestion. These are points to be inquired into and will be of curious interest. Still it is certain that in diseased conditions of the stomach a whole series of diagnostic symptoms will be learned, bearing not only on the fermentative action in progress, but on alterations in the walls, and on contractile functions. The stomach, in short, will admit of being sounded like

the chest. Scirrhus affecting the stomach should easily be diagnosed by auscultation. Through the stomach it may also be possible to diagnose physically and more correctly than we now diagnose the nature of some pulsating abdominal tumours which are, as they have been since the time of William Harvey himself, a constant source of disagreement amongst physicians. With the terminal of a full-sized œsophageal stethoscope in the stomachic cavity a loud murmur from an arterial source will be detected without the interposition of pressure, and an important difficulty in diagnosis removed. A third service that may be rendered by intra-thoracic auscultation is forecast in the observation I have already made respecting the detection of the cardiac sounds in conditions when those sounds are inaudible under the usual method owing to extreme feebleness of the circulation. We are called sometimes to persons in such entire collapse that it is doubtful whether they are alive or dead. The sounds of the heart are imperceptible to the ear through the thoracic walls. Here, then, is an instant and ready method of deciding whether the action of the heart is still in progress. We have a new proof either of absolute death or of continued life. In acute collapse, as after death from chloroform, we not only can institute a good diagnosis in the same manner, but we are half-way, by our manipulation, towards assisting to restore life. Through the œsophageal tube, after disconnecting the stethoscopic tubes, we could inject a free quantity of heated water or water with oxygen peroxide into the stomach; or, if we had learned the practice correctly, we could direct an electrical current upon the heart itself, so as to excite contraction of the right side.

The use of the intra-thoracic method may be turned to account in diagnosis of heart disease and of aneurism of the large thoracic arterial trunks. Stricture of the œsophagus arising from aneurismal pressure would be instantly diagnosed. These cases are often most perplexing. The pressure causing the obstruction varies, and no sufficient sign of aneurismal murmur in the early stages reaches the ear at all times. One auscultator fortunately catches the murmur, another does not, and so contradiction upon contradiction amongst the best diagnosticians occurs. By the process of intra-thoracic auscultation this difficulty would be immediately met, the pulsation would come under direct observation, and the precise seat of the pressure would be descended upon. We should hear a pulsating stricture. In heart disease itself the intra-thoracic mode of examination should be of service. By it we ought to be able, in cases of enlargement, to distinguish between dilatation and hypertrophy. The diagnosis of clot on the right side might be made clear by this plan, and the differential diagnosis of valvular affection on the right or left cavity ought to be rendered absolute, when we have learned sufficient of œsophageal auscultation to discover the new distinctions of sound that will have to be made, in some cases at least, between the tone of the first and the second sounds, and have defined all new lines of distinction between these sounds as heard from within and from without the exterior walls of the chest. The clearest definition by this test should be obtainable also as between pericardial and endocardial friction sound and between pericardial as distinct from pleural friction.

As this is only a preliminary note, I must leave untouched the subject of pulmonary auscultation by

the intra-thoracic method and also that of pharyngeal exploration. The pharynx is within such easy reach, it should admit of more frequent auscultation than lower parts of the œsophageal canal. Its size, too, renders it more accessible. From it we should be able to reach the apices of the lung and the greater part of the air passages from the larynx down to the bifurcation of the bronchi. Exploration of these parts will be easy when a set of appropriate and convenient exploring instruments has been constructed, and will afford help to diagnosis of changes incident to the apices of the lungs in subclavian, innominate, and carotid murmurs, and in thickenings and obstructions of the trachea and larynx.

Having given an outline of what may be gained by intra-thoracic auscultation, I shall now refer to the limitations of the plan and to the objections which may be taken to it. I assume at once that this mode of research is not called for when by the ordinary auscultation diagnosis is clear. It may come in usefully in all cases where the œsophageal tube is used for exploration, but it will not be always applicable in instances where it might be useful, owing to the circumstance that many patients are unable to bear the introduction of the tube, and that it may sometimes be unadvisable to subject them to it. I met with a patient last week suffering from probable thoracic aneurism in whose case the exploration would have been most valuable; but the introduction of the tube caused so much retching and straining, I was obliged to withdraw the tube before I had explored as fully as I could have wished, although what was effected was sufficient to inform me of the existence of aneurismal murmur from the left carotid or subclavian. The explorations might also be objectionable in cases of

irritable stomach, or where there was severe cough or cough with hæmorrhage. I need not dwell any longer on particular points of this kind, since the whole is summed up in a sentence: whenever the practitioner feels he can safely and prudently pass a tube into the stomach, he can practise intra-thoracic auscultation.

The third point, the modes in which this method may be improved by mechanical means, opens up a very wide field of inquiry. I commenced my work with the ordinary flexible tube, and up to the present time I have found nothing better than a good-sized tube with a large lateral aperture at the extreme end. I have used another tube charged with several apertures an inch from each other, and this answers fairly well. Apertures are essential in these tubes; if they are not made there is little or no conduction of sound. Messrs. Krohne and Sesemann have been making tubes for me of different materials, and one of metal of malleable nature is good in many ways; but I have not yet obtained what is precisely wanted. The best tube at this moment is the long œsophageal flexible tube with the stethoscope attached to it.

I leave now this contribution in the hands of a society to which it has been my high privilege to communicate many of my first thoughts from the very opening of my long career. I would not on any account attach an undue importance to the effort, or look upon it as anything more than an extension of the simple act of the illustrious Laennec, when, in the Necker Hospital one day in 1816, he improvised a stethoscope out of a roll of paper and projected thereon a new science to which there seems to be no end.

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."

BENJAMIN RUSH.

AN EPIDEMIC CATARRH.

IN the latter part of last October a widespread catarrh extended through London. It was scarcely sufficient to cause any one to keep the house, but it was very troublesome and enfeebling. It attacked in an insidious manner. The first symptoms were those of loud and, one might almost say, explosive sneezing, unaccompanied by any of the usual signs of what is commonly called cold in the head. There was no frontal headache, no pain in the back or limbs, no depression, but a continued irritation, with a free watery discharge from the nose. In a few hours the irritation descended to the bronchial surface, and then there occurred a hollow bronchial cough, with a fluid and somewhat copious expectoration, which lasted for several days. There was no febrile exacerbation, no soreness of the throat, no disturbance whatever of the alimentary canal, and no muscular pain; nothing that kept the majority of sufferers from their usual occupations; each attack passed off, even though the weather was unfavourable, without any organic mischief. If my observation be correct, the day on which the symptoms broke out was Tuesday, October 25th, and in the succeeding two days the disease was widespread. There was not the slightest evidence that

it was contagious; it affected particular members of families, but children were rarely, if ever, affected by it. Indeed, it was so temporary, it might have escaped notice altogether but for the numbers of persons attacked. I read a statement by a practitioner of medicine that in his observation two persons out of three were affected by the catarrh. I have myself no sufficient facts to confirm this view, and I do not know how they could be precisely obtained; but the statement at least confirms the impression of the widely diffused character of the epidemic. It is worthy of note that I did not meet with one instance of this catarrh in the wards of the hospital—an immunity due probably to the equable warmth of the air of the wards and the protection from variable conditions of air. There were at the time, however, several relapses in the convalescent rheumatic and neuralgic cases. Whatever the affection arose from, the effects were purely local, as local as the effects of snuff on those not accustomed to take snuff. Some changed condition of air was present which irritated in a vibratory or mechanical manner the sensitive Schneiderian surface, and extended its action, in a limited degree, to the trachea and the upper portion of the bronchial tract.

The epidemic passed away in London as suddenly as it commenced. I do not think its period lasted more than ten days, and it certainly left none of the serious after-effects which were observed in the train of the epidemic of influenza. It caused no obvious increase in the mortality of the people, and recovery from it took place without the application of any particular line of treatment, hygienic or curative.

NOTE ON PEROXIDE OF HYDROGEN IN DIPHTHERIA.

I AGREE with Dr. F. H. Williams, of Boston, U.S.A., as to the value of a thirty-volume solution of the peroxide of hydrogen in the local treatment of diphtheria, as described in the *Medical Press and Circular* of October 26th, p. 436. I wish, at the same time, to add to this commendation the important warning that the thirty-volume solution should never be used except on open surfaces. In 1857, when I began to experiment with it, the peroxide was the rarest of chemical curiosities; it had never been used in medicine, and I had not a spark of light to guide me as to the number of volumes that could safely be employed medicinally. I began with strengths of four and five volumes; then I moved up to twenty and thirty volumes; but I soon learned that with the higher volumes the oxidation was so rapid in the presence of pus and similar disturbing substances, that the effect was practically explosive in character. In a case of abscess of the antrum I injected a drachm of a thirty-volume solution through an opening into the cavity made by the extraction of a tooth and free perforation, and witnessed an action which for a few seconds alarmed me, owing to the gush of purulent foam that followed. I found afterwards that for the destruction of pus weaker solutions would answer well enough, and from that time until my first paper on the substance read to the Medical Society of London in 1860 I gave to this question of volumes the most careful study. In the end I came to the conclusion that on the whole the ten-volume strength was the most practical, and I fixed on that as a standard which has been generally adopted by the

profession. I have never seen occasion to suggest the alteration of that standard, but there are exceptional cases where a solution of greater strength may be used, and diphtheria is one of these. By the local application of the thirty-volume solution to destruction of the diphtheritic membrane and adhesions of it are more rapidly secured than by the lower volumetric strength, and, as the surface is open, there is no danger of creating tension or forcible rupture of parts. The same rule applies to the use of the thirty-volume solution on the cutaneous surface in phagedæna, syphilitic sore, and senile gangrene.

On the other side, there are cases in which the ten-volume standard solution may be advantageously used in very small quantities. I have a case in hospital just now which illustrates this point. A woman is suffering from a circumscribed abscess discharging from a fistulous opening in the abdominal wall. Here I introduce the solution in small quantities by saturating a pledget of cotton with the solution, introducing it through the sinus by the probe, and repeating the dressing frequently. By this plan I have seen a large cavity close up in the most satisfactory manner. In the treatment of *fistula in ano* this method ought often to supplant operation by the knife.

THOMAS SYDENHAM, M.D., AND HIPPOCRATIC PHYSIC.

IN the three last numbers of ASCLEPIAD I have studied three historical characters, all of one nation, and all belonging to one peculiar and critical time in the history of the English people and of medical art. Thomas Willis, Kenelme Digby, Thomas Browne, have passed before us: the first, the profound physiological physician; the second, the versatile scholar in philosophy, including physic in some of its departments; the third, the antiquarian, and I had almost said theologian of medicine, the poet and the practitioner, strange as the combination was and ever will be—Sir Thomas Browne.

I pass now to a man of the same time, a man of very different quality of mind from either of his distinguished cotemporaries, a man who has made in the family of medicine by far the deepest impression; a man whose practical work is still in our exclusive fraternity a household word—Thomas Sydenham. Why Sydenham has left so indelible a mark in medicine is a question we shall have to discover in the course of this essay. He had none of the genius of Willis, or the learning of Browne, and yet his name is spoken now a hundred times to one compared with either, nay

both. In fact, dead and gone as he is, he lives yet amongst us. We gild a society with his name. An American physician of the past rejoiced most when he was designated the modern Sydenham, as Sydenham might have rejoiced at hearing himself called the modern Hippocrates. I remember the late Sir Thomas Watson being compared to Sydenham, and he told me that he had never felt himself more honourably mentioned.

There must have been some remarkable mind in this man to win such ever-progressing admiration. Let us look at him in his mortal life as best we can.

EARLY CAREER.

It is agreed on all sides that we have very little knowledge of an exact kind of the early career of Sydenham. We know that his father, a gentleman of good position, was William Sydenham, of Winford Eagle, in Dorsetshire. His mother was Mary Geffery, daughter of John Geffery, of Catherstone. He had five brothers: William, the eldest, Francis, John, George, and Richard; and three sisters: Mary, Elizabeth, and Martha. He himself was born in 1624, at a time when his parents had been married thirteen years. The family seems to have been parliamentarian in a political point of view, and to have fought well on the parliamentary side. William became a colonel in the parliamentary army, and was for a time Governor of the Isle of Wight and Vice-Admiral of that island and of Hampshire. Francis was major of a regiment, and, as Dr. Nias has recently shown, John and Richard were both captains on the same side. One of his sisters, Martha, married into a parliamentary family, being the wife of a lawyer named Lawrence, a staunch republican.

It is a curious circumstance that the mother of Sydenham and his brother Francis were both killed in the wars, but not at the same time. In some way Mrs. Sydenham was killed by a royal officer, one Major Williams. Afterwards, in November 1664, a fight took place at Poole, in Dorset, between the King's troops, under the command of Sir Lewis Dives, and a body of parliamentarians, under Major Francis Sydenham. In one of the charges Major Sydenham recognised Major Williams, by whom his mother had been slain, and, coming up to him, slew him and put his men to flight. In the same year he (Francis Sydenham) was himself slain.

It is supposed that our Sydenham in his early life had charge of a troop of horse on the republican side; and, as Dr. Gordon Latham skilfully contends, there is excellent reason for believing this to have been the case, but proofs of a direct kind are still wanting. We get a glimpse of our man as a youth of eighteen years of age at Magdalen College, Oxford, in 1642, but he did not remain there for a long period. Oxford became a garrison of the King, Charles I., and the centre of the Court. Harvey and Willis and Digby were there as staunch royalists; but Sydenham left for London, "and did not," says Ward, "bear arms for him as other scholars did." He came to London, where his brother William was then located, and while there met with a physician, Dr. Coxe, who was in attendance on that brother in a serious illness. Coxe made, it is thought, a great impression on Sydenham, and persuaded him to give up everything in order to devote himself to the study of the science and art of medicine.

When, after the great fight at Naseby (1645) Oxford

surrendered and the King's cause was lost, Sydenham returned to the University city and resumed his studies at Magdalen, but only for a short period. There was a great turn-out of the royalist Fellows and Dons in the colleges, with substitution of men who were on the popular side. Sydenham shared in the spoil. He was elected a Fellow of All Souls' in place of a royalist Fellow whose name does not transpire, and he at once took residence in his new home, and began to study physic, it would seem exclusively, for he took no degree in arts, but in 1648 went up for his Bachelorship of Medicine and was duly admitted.

For some years, it is not very easy to say how many, he remained in Oxford, it is most likely in All Souls' College as a Fellow. From a statement made by Desault it is believed by some that he went through the process of education, then common in medicine, of visiting Montpelier and studying at that far-famed school. Dr. Latham accepts this view, but there is nothing in the writings of Sydenham that sustains the idea, which is at least remarkable.

Sydenham lived in Oxford until a little time before the death of the Lord Protector, an event which occurred in 1658. Until lately all the biographers have agreed that he did not settle in London until the year after the Restoration (1661), but Dr. Gee and Dr. Nias have recently fixed the event for 1656. He must have been married soon after he came to London, and from the fact that he left an annuity to his wife's mother, Mrs. Catherine Gee, it is implied that his wife's maiden name was Gee. By her he had three sons—William, Henry, and James—the first of whom entered at Pembroke College, Cambridge, in 1674, and followed his father's profession.

He settled first to practise in Westminster, in a house on the site of Ram's Mews, in King Street. Afterwards, on the Restoration, he removed, in 1664, to a house on the north side of Pall Mall, next to the sign of "The Pestle and Mortar," where he remained until 1689. His practice here was great; and there also he had pupils, including Hans Sloane, the future founder-in-chief of the British Museum, Thomas Dover, the inventor of the splendid preparation that still goes by his name (Dover's powder), and Bartholomew Beale, son of Mrs. Beale, an artist of much renown.

It has been brought against Sydenham that he was one of those who ran away from the Plague of London in 1665. Dr. Nias explains on this point, if it be true, that at the time he would be "living in a neighbourhood which the inhabitants had deserted, and was therefore probably left with but little to do. Also his wife must have been bearing him a family, which was another reason for going."

Two years before the advent of the Great Plague, namely, in 1663, he appeared before the Censors of the Royal College of Physicians for the license of the College. He was examined at the College on the 24th of April, the 8th of May, and the 5th of June for the license, and was granted it on the 23rd of June, 1663. At that time he would be thirty-nine years old, and would be fully embarked on his professional career as a metropolitan physician. That he rose quickly into fame is testified by all cotemporaries. "He lived," says one of his biographies, "in the first degree of reputation and in the greatest affluence of practice for many years, without any other enemies than those which he raised by the superior merit of his conduct, the brighter lustre of his abilities, his improvements of

his science, and his contempt of pernicious methods, supported only by authority, in opposition to sound reason and indubitable experience."

He was clearly a man of strong determination and of original observation in his own ways of thought and action. He wrote from what he saw in medicine, and his sphere was the sick-room. The sick were always on his horizon. He had not many remedies, and what he had he used sparingly; but he did his work with precision, and therefore with confidence. He read few books written by men, but he read men as books, wherein lay his original power. Sir Richard Blackmore, who knew him, tells a story of him that once when he (Sir Richard) asked him what books he should read to qualify him for medical life, he got for reply: "Read '*Don Quixote*'; it is a very good book; I read it still." This was not exactly advice; it was humour bordering on gentle satire. Sydenham knew his man, and bade a Quixotic nature follow its own bent. He held no public position in any of the hospitals of the city, and, singularly enough, took no rank beyond that of a Licentiate in the Royal College of Physicians. He did not seem to be ambitious for any post of honour. Sir William Browne dwells on this point, declaring that "the order of Licentiate of the College was a title high enough to content the great Sydenham, our British Hippocrates, to his death. Nor did he think it an indignity to his doctor's degree, or to the University that conferred it; nor did he choose to exchange it for a higher, so remarkable was his modesty as well as his ability; although our Society—the Royal College—would have received with open arms one who was so great an ornament to it even as a Licentiate—one who must have added even to its highest honours."

I quote this passage in full in order to sustain what the learned Harveian Librarian of our day, Dr. Munk, has to say bearing upon the vexed question of Sydenham's relationship to the Royal College. Lettsom affirmed to a past generation that Sydenham was visited by the envy and enmity of the Fellows of the College. "I am unable," says Munk, "to discover any grounds for the assertions of Dr. Lettsom. That Sydenham had opponents, and even enemies, among his cotemporary physicians, his own reiterated statements forbid us to deny. There is certainly no reason to suppose that such feelings attached peculiarly to the Fellows of the College—much less that it influenced them in their collegiate capacity. On the contrary, whenever Sydenham is mentioned in the Annals, it is in terms of respect and admiration." This view, the same author contends, is supported by the wording of a grace of the College granted to Sydenham when application was made by him for permission to print a second edition of the "*Schedula Monitoria.*" Permission was given "*lubentissimè*," most willingly—a term used quite exceptionally on such appeals. It is obvious that Sydenham's failure to be enrolled a Fellow of the College was not from mere envy, nor from any special enmity of the College in its corporate capacity; neither need we attribute it to any special modesty on his part, since modesty overstrained may become an obstacle to the usefulness an able and active man may render to a good cause. I take it that the reason was really political. When Sydenham graduated Bachelor of Medicine, in 1648, King Charles was on his defence, and the Parliament was taking unto itself the supreme power. Five years later Cromwell was at the head of affairs, and remained in that position until his death

in 1658; in 1660 Charles II. was restored as king. Sydenham had resided in Oxford during almost the whole period of the Commonwealth, and part of the time at least as a Fellow of All Souls' College, as a Commonwealth Fellow. It is not unfair to conclude that on the Restoration his connection with All Souls' was broken in favour of a royalist, and that, coming to London, as he did, very shortly before the resumption of royal authority, he would be at a distance with all the royal institutions, of which the Royal College of Physicians would be amongst the most prominent. We could see, too, there would be difficulty in admitting him to the Fellowship. He had a right to ask examination for the license, but for an honour which came by spontaneous selection and grace he could scarcely look, and he would be too proud to solicit it. He would therefore be content with that which he had won, and would rest on his own ability for the success he wished to achieve. For the same reason he would not seek admission into the newly formed Royal Society, although one of his papers received notice in its Transactions. In brief, a Commonwealth man and physician in the midst of a royalist community, elate with the Restoration triumph, would have enough to do to hold his own, without striving to add to his name an accumulation of adverse honours. Moreover, he was just the man to be honest in his convictions, and accept nothing from favour that he could not hold from conviction. Let then all the scandal—for it is nothing less—respecting Sydenham and the Royal College of Physicians pass into silence. Let puritan Lettsom himself be silenced. Sydenham was a martyr, if there were any martyrdom in the case, to the force of circumstances over which he had no control, not to any malevolence

of his compeers. Every man of good mark suffers more or less from force of circumstances, just as some men of no mark at all, or less than no mark, rise from force of circumstances, and are lifted *nolens volens* into an eminence to which they have no right either by ability or good reputation.

One other difficulty, technical but powerful, stood also in the way of the College. The statutes of the College ordained that for a physician to be raised to the Fellowship he must be a Doctor of Medicine. He could not even be a candidate with a lesser degree. But Sydenham came forward simply as a Bachelor of Medicine to London, and was content to enter into and continue in practice on that degree. He was not, therefore, for many years eligible for the Fellowship; and when, in 1676, he proceeded to take his Doctorate, not from Oxford, but from Pembroke College, Cambridge, he was so established in fame that it was of no moment to him to possess new distinctions or honours. There is nothing lost. If we Fellows of the Royal College regret the absence of his name from our lists, the Licentiates have their revenge. His name immortalises their calendar.

THE WORKS OF SYDENHAM.

"METHODUS CURANDI."

The works of Sydenham are not numerous. Wallis puts them, in his translation, into two volumes of 480 pages each; and in my comments I shall follow chiefly his copy. In the Royal College of Physicians there is a MS. copy in English of his "*Observationes Medicæ*," presented on November 17th, 1795, by Mr. Paul Vaillant.

This was not known to Wallis, but it does not add anything new.

The first great work of Sydenham, published in 1666, "*Methodus Curandi Febris Propriis Observationibus Superstructa*," is enriched by a preface. A preface may be the most important part of a work, and in one sense it is so here. The preface shows us the mind of its author. He would always, he says, have esteemed himself a useless member of society had he not communicated his mite towards the improvement of physic; and upon this account, after long deliberation and many years of close and faithful observation, he resolved to communicate his thoughts relating to the manner of making farther advances in physic, and of publishing a specimen of his endeavours in this way. Having thus declared his object, he next states his opinion as to the method in which physic is to be advanced; and it is at this point he offers the method so peculiar to himself. It is here that he falls back upon the Hippocratic line of research. Here he affirms what would be to-day as novel as it was in his own day. Suppose a teacher were now to rise and affirm: "You of to-day are experimental, physiological, abstruse, laboured. You strive to find out in the domain of disease methods that shall be applicable to the treatment of disease. You are wrong, all of you: disease can only be studied, comprehended, mastered, by keeping your minds concentrate on the field of disease. Give up experimentalising, give up speculation, give up all exoteric influences; keep to your text in its purity, the natural study of disease itself. That is what Hippocrates did; that is what you must do. The fathers of physic had no knowledge of your methods; therefore it was impossible for them to formulate any practice on your methods. They saw

disease as an aberration from healthy nature, and they tried to put things right by what they learned from direct observation. You must do the same." Suppose a man declaring this at the present hour. That man would be the echo of Thomas Sydenham. Here are his words in their integrity:—

"The improvement of physic, in my opinion, depends:—(1) upon collecting a generic and natural description or history of all diseases, as far as can be secured; and, (2) laying down a fixed and complete method of cure."

This is the keynote of the Sydenham practice of medicine. What should we say to it in this day? But we must follow it up on its original basis into details:—

"All diseases ought to be reduced to certain and determinate kinds, with the same exactness as we see it done by botanic writers in their treatises on plants. The distribution of distempers into kinds must not be done to serve the purpose of some hypothesis founded upon the true phenomena. To this cause is owing that the materia medica is so immensely enlarged, and yet with little advantage to the diseased."

"In writing the history of diseases every philosophical hypothesis which has possessed the writer in its favour ought to be totally laid aside, and then the manifest and natural phenomena of diseases, however minute, ought to be noted with the utmost accuracy, imitating in this the great exactness of painters, who in their pictures copy the smallest spots or moles in the originals. Again, in describing any disease, it is necessary to enumerate both the peculiar and constant phenomena or symptoms, and the accidental ones separately; of which latter kind are those which differ occasionally by reason of the age and constitution of the patients, and

the different methods of cure. Lastly, the seasons of the year that principally promote any particular kind of diseases are to be carefully marked. It is to be owned that some diseases happen indiscriminately at any time; whilst many others, by a secret tendency of nature, follow the seasons of the year with as much certainty as some birds and plants."

So much for the study of disease. "But a physician may likewise collect the indications of cure from the smallest circumstances of the distemper as certainly as he does the distinguishing signs from them. By these steps and helps the Father of Physic, the never-enough-extolled Hippocrates, came to excel in his profession." If any one were to ask whether, besides the two foregoing desiderata in physic—namely, a true history of disease, and a certain and established method of cure—a third should not be added—namely, a discovery of specific remedies—he answers in the affirmative, and promises to do his part towards the effort; "for though the best art in curing diseases consists in assisting nature to cure by evacuation, it is nevertheless to be wished that the cure might be shortened by means of specifics (if any such medicines can be discovered), and, which is of more importance, that the patient may be preserved from the evils which are the consequence of those errors that Nature often unwillingly makes in expelling the cause, even though she be assisted in the most effectual and skilled manner by the physician."

The above remarks of the master apply only to the treatment of acute diseases; when chronic diseases are being considered, another element comes in. Nature then is not possessed of so effectual a method of expelling the morbific matter. In eradicating a chronic disease, therefore, whoever is possessed of a medicine powerful

enough to destroy the species of it justly deserves the appellation of physician.

Specific Remedies.

Regarding specifics, Sydenham held that there was one alone—namely, the Peruvian bark. "For there is," he argues, "a wide difference between medicines that *specifically* answer some certain curative indications and those that specifically and immediately cure a disease without regarding any particular intention or curative indication." To exemplify this, he says that "mercury and sarsaparilla are usually reckoned specifics in the venereal disease, though they ought not to be deemed proper and immediate specifics unless it could be demonstrated by undeniable instances that *mercury* had cured the patient without causing a *salivation*, and sarsaparilla without causing a *sweat*. For other diseases are cured in the same way by other evacuations, and nevertheless the medicines exhibited for this purpose do no more immediately contribute to the cure of the diseases that yield to those evacuations than a lancet does to the cure of a pleurisy, which nobody will call a specific in that particular disorder."

It is remarkable how little we have progressed in our work at specifics since the day when Sydenham wrote the passages here cited. To his one specific —Peruvian bark—we have added potassium iodide, and possibly sodium salicylate. There we stand, and little better. It is clear that Sydenham had gauged the matter of cure by common remedies and specific remedies with shrewd intelligence, and the test he put is singularly happy. Does mercury cure venereal

disease by a specific virtue of its own as mercury, or by its eliminating effect as a producer of salivation, an eliminator? We could not answer that question even now. If it cures by salivation, as an eliminator, it is a common evacuant; if it will cure less salivation, it is a specific. Who knows the secret?

Vegetable Remedies.

As a prescriber Sydenham inclined to remedies derived from the vegetable kingdom. "It is indeed a pity," he writes, "that we are no better acquainted with the virtues of plants, which I esteem the best part of the materia medica, and most likely to afford such medicines as we have just treated of. For the parts of animals should seem to resemble the human body too nearly, and minerals to differ too much from it; and hence it is, I ingenuously own, that minerals more effectually answer indications than medicines prepared from animals or plants do, but yet not specifically, in the sense and manner above mentioned." It is worth our thought whether this master may not be correct in this particular. Parts of animals are dead: they have passed through their vital services; parts of minerals are dead: they have not entered into the realms of life. How can either be specific remedies? Parts of plants are in the domain of life, but have reached only the stage when from them the animal structures can be formed and the animal powers vitalised. They are in the condition for specific purposes, and if we knew how to use them would indeed probably yield us the virtues we require for specific treatments.

It must be borne in mind that the preface referred to above is written as preliminary to the author's

method. I have tried to put into a nutshell the foundations of his method and practice. Those foundations were laid soundly as far as they went, and were described honestly and boldly, but it is certain they were not popular in the ranks of the profession. They were in advance, and nothing is so unpopular and unpropitious to a man as advancement in independent form. He is a fool and blind to the evidence of history who, whatever his genius, foresight, or industry may be, expects to keep on cordial terms with his cotemporaries when he is everlastingly running ahead of them and bringing back new observations. It is not in human nature they should like him; it is in human nature, first and foremost, that they should treat him as a kind of deserter so self-willed and independent, there is no dealing with him. Sydenham knew and felt all this; and dropped naturally the proud lamentation that, "sensible of exposing all the fruit of his labours for the best part of his life to the intolerant and ignorant, expecting from the disposition of his degenerate age nothing but censure and contumely in return, and knowing he could have gained more reputation by advancing some trifling and useless speculation, he hopes still *to be rewarded elsewhere.*" Sydenham has been rewarded elsewhere. He has been rewarded by the posterity into which he cast himself. He wrote these sayings in prophecy with a full heart. They were puritan sobs, but they had better not have been expressed. What did it signify to Sydenham that the "ignorant and intolerant" of his time subjected him to censure and contumely? He merely flattered their ignorance and intolerance by showing them he felt it. He had better have passed them by like low brawlers at the corners of the king's highway, and,

heeding them not, pursued his own course to the gates of paradise.

In his conclusion, Sydenham observes that he knows men as well versed as himself differ from him, but it is not his business to inquire what other persons think: it is his business to establish the truth of his own observations, in doing which he prays for the patience of his readers, not their favour. He renounces "the pompous part of prescription," and follows Hippocrates in simplicity. Let us take him at his word.

DISEASES, ACUTE AND CHRONIC.

In the opinion of Sydenham, a disease, however prejudicial its causes may be to the body, is no more than a vigorous effort of Nature to throw off the morbific matter, and thus lead to the recovery of the sick. The causes of acute disease, according to him, are twofold. In one kind particles of the air which disagree with the "juices" of the body insinuate themselves into the lungs and permeate the body, mixing with the blood; in the other kind some fermentative changes take place in the body, with putrefaction processes, and thereupon the body, being unable to eliminate the poison, becomes deranged or ill, and Nature therefore provides such a method or train of symptoms as may expel the peccant matter, that would otherwise destroy the human fabric. By this Nature sometimes effects a cure when left to herself; yet not always, for sometimes too, either by endeavouring too much or not enough, the patient pays the debt of mortality: "*for it is an immutable law, that no generated being can always continue.*" In these views Sydenham really expresses what we teach now. His

diseases from bad air are our diseases from the same cause; his fermentative diseases are our zymotics; his diseases from products of fermentation and putrefaction are our auto-toxæmias and blood poisonings. That which he missed in his system or method was the influence of vibrations and the nervous reflexes. Hartley had not lived as yet, and Sydenham had no such originality or training as would lead him to understand the power of the mechanical forces of the universe on the human organism.

The division of disease into acute and chronic forms depends, after the views advanced by Sydenham, if I understand him correctly, on the fact of the presence or absence of fever. That which is acute is febrile. If the body be urged by natural forces into excitement and fever in order rapidly to destroy or remove poisonous material, *materies morbi* that would kill if they remained even for a short time in the body, then the disease is an acute disease. But if the cause at work to produce disturbance be so inactive as not to light up excitement, if it become, as it were, adapted to the body, making it ill without immediately destroying it, then the disease is a chronic disease, and requires treatment as distinctly applicable to its condition as the acute affection does in its way. Wallis attempted to simplify this reading of Sydenham by generalising upon it, but I do not feel that he succeeded, while he certainly lost in refinement and precision. In chronic disease, says Sydenham, the cause of the disease is fixed in the particular part, and cannot raise the assistance of a fever or of febrile heat to expel it, so it remains there. Thus in palsy the morbific matter is fixed in the nerves, and the bodily forces are not efficient for conveying it off. Change terms a little,

and the theory holds. After sciatica *sans* fever, why is there often the numbness and partial paralysis of limb? What better theory than that, in the course of the imperfect nutrition of the affected nerve, some change of structure takes place in it, or some new substance is deposited in it, which afterwards interferes with function, as if the nervous trunk had been physically injured? In acute diseases the change leading to chronic effects may be peremptorily checked by the very acuteness of the attack, a natural explanation of what we call *crisis*. The cause removed, recovery sets in.

EPIDEMIC DISEASES AND EPIDEMIC INFLUENCE.

I am not quite sure whether the view put forth by Sydenham in regard to epidemic diseases and epidemic influence was or was not entirely original. He is generally credited with real originality of thought on the matter, but in truth Willis and other of his contemporaries expressed, in their way, similar doctrine, and I feel candidly that in what he says he is rather expressing the advanced thought of his day than anything particularly and from the first his own. At the same time it is well to know precisely what he did say; and this is it. He said that "some acute diseases proceed from a latent and inexplicable alteration of the air, infecting the bodies of men; and not from any peculiar state or disposition of blood and juices, any further than an occult influence of the air may communicate this to the body; the derangements continue only during this one secret state or constitution of the air, and raging at no other time, are called *epidemic distempers*."

In these words the whole of the Sydenham idea of

the originating cause of epidemics is generally embodied.. When he descends to particulars he gives us fresh notions as to cause. "There are various constitutions of years that owe their origin neither to heat nor cold, dryness nor moisture, *but rather depend upon a certain secret and inexplicable alteration in the bowels of the earth*, where the air becomes impregnated with such kinds of *effluvia* as subject the human body to particular distempers so long as that kind of constitution prevails, which, after a certain course of years, declines, and gives way to another. Each of these general constitutions is attended with its own and peculiar kind of fever, which never appears in any others; and therefore," says he, "I call this kind of fever stationary."

In every year, however, other fevers arise, which proceed from some one or other manifest quality of the air; for instance, pleurisies, quinsies, and the like generally happen when an intense and long-continued cold is immediately succeeded by a sudden heat. These diseases he calls the "*intercurrent*," as distinguished from the "*stationary*." "The sensible qualities of the air have thus some share in producing these intercurrent fevers, which appear in every constitution of the atmosphere. They do not cause the epidemics peculiar to the constitution, yet they may more or less dispose the body to the particular epidemic disease.

"Some epidemics in some years are uniformly and constantly the same, appearing in almost every patient with the same train of symptoms, and going off in the same manner. From this kind, therefore, as the most perfect, the general history of epidemic diseases is to be taken." On the contrary, in other years other distempers prove irregular and dissimilar, as having

no one fixed form; and there is another subtilty of nature still behind—viz., that the same disease, in the very same constitution of the year, frequently appears in a various and dissimilar manner as to the time of its beginning, state, and declension. Again, all epidemics are of two sorts, vernal and autumnal. Measles and the vernal tertians begin before the vernal equinox, commencing in January and disappearing about the summer solstice; whilst others, like the plague and small-pox, rising in the spring and daily increasing, come not to their state till about the autumnal equinox, after which they gradually decrease, and vanish at the approach of winter. "*Cholera morbus* rises in August, and finishes its course in a month; but dysentery, rising at the same time as autumnal tertians and quartans, runs on to the winter."

It must be carefully remarked that, as many of the diseases named appear in the same year, some one or other of them rules over the rest, which rage less at the same time; so that, this one increasing, the others decrease, and, this diminishing, the others soon reappear; and thus they prevail by turns, according as each is favoured by the disposition of the year and the sensible qualities of the air. But all epidemics of one constitution are produced by one common general cause; and all epidemic distempers, especially fevers, "*depend upon the secret constitution of the air.*"

These general principles are illustrated by an account of the epidemic constitution of the years 1661-4 in London. The description of the symptoms and history of the whole series of diseases furnishes a narrative of the first order, even as a piece of human history. I must refer the student to the work itself for details, and when he is engaged in tasting from the original spring,

I am sure he will not regret my inability to find room for what at best could be but an imperfect epitome.

I pass over sundry other essays and epistles—"*A Treatise of the Gout and Dropsy*," the "*Schedula Monitoria*," an essay "on the rise of a new fever," a treatise on "the small-pox," a treatise on "the history and treatment of the venereal disease," and a dissertation on "bloody urine"—in order to arrive at what he called the "*Processus Integri*," or complete methods of curing most diseases, to which he added an accurate description of their several symptoms, many judicious observations, and a short treatise "of a consumption." This last work, "*Processus Integri*," was drawn up by Sydenham for the use of his son, and was edited by "S. M." after his death. The "*Processus*" is, in fact, a compendium of Sydenham; it gives his diagnosis, his prognosis, and his therapeutics, all in a short and practical form. It begins with a list of the prescriptions commonly used by Sydenham, and it continues with the account of the special remedies and regimens which he was in the habit of employing. By a perusal of this work we get an insight into the practice of the man at the bedside. We learn that his predilection was all for plant remedies; we see that, whilst he handled some potent remedies well, he was deceived constantly as to other remedies in which he had faith; and we gather that his regimen was often far ahead of his physic. Some of the remedies sound to our modern ears singular. The distilled water of holy thistle, the distilled water of black cherries and milk, the conserve of red roses, garden scurvy grass and sage, syrup of buckthorn,—these, and many other substances equally simple, are introduced as if their curative value were unimpeachable. I notice, however, at the same time, that he

often mixed with these simples other potent drugs—antimony, opium, rhubarb, and iron; but he held that it was not safe to give antimonial wine, even in the smallest dose, to children under fourteen. In nearly all diseases he drew blood, and in intermittent fevers he had learned to trust to bark; but in the "stationary fever of 1685-90 he recommended repeated bleedings with purgation" till the patient recovered. For scarlet fever, which he differentiated from measles, he prescribed burnt hartshorn in powder,—not a bad remedy; for pleurisy, bleeding to ten ounces, followed by pectoral remedies; for acute rheumatism, bleeding, followed by syrup of lemons and violets mixed with distilled waters, which may be "drank at pleasure"; for erysipelas, bleeding and purging; for "stubborn itch," the same; for small-pox, the same, with purging and with opiates, which are always to be used in this disease; for St. Vitus' dance, again, bleeding and purging; for apoplexy, immediate bleeding; for the cholera morbus, opium; for profuse menstruation, bleeding and purging; for vomiting and spitting of blood, the same.

Sydenham was quite aware of the difference between a gonorrhœa virulenta, or "venereal running," and the venereal disease; but he considered the distinctive venereal disease only arose "when the blood is tainted by the long continuance of a gonorrhœa, or the ill-advised use of astringents when the true pox appears, attended with buboes in the groin." The error extended down to the time of John Hunter, and was not even detected by him. Sydenham did not detect it, but he knew all the symptoms of venereal disease in every stage, to the stage "when the nose falls flat, the ulcers caries and pains increase every day, the limbs rot away piecemeal, and the mangled carcase, being at

length grown hateful to the living, is buried in the earth."

Sydenham's treatment of venereal disease was mercury by inunction up to quick salivation, the salivation being so regulated "that the patient may spit about two quarts in twenty-four hours." Afterwards, to prevent a relapse, "the patient took a scruple of sweet mercury once a week for a month or six weeks, though he appeared to be perfectly well, and able to go abroad."

For chlorosis Sydenham administered iron with wine and roots of angelica. This was accompanied by purges. Here we almost follow Sydenham at the present day. Gout, from which he himself was a great sufferer, was, he thought, best treated by a simple and very temperate regimen, in which he is very sound. He did not approve of bleeding and purging for this affection. In the early stages of true consumption of the lungs he bled moderately to allay acute symptoms; but, he says, "the principal assistant in the cure of this disease is riding on horseback daily"; nor need the patient be tied down to any particular rules of diet, "as the cure depends wholly upon exercise." Lately we have returned to the Sydenham cure for consumption, and our modern practice confirms his. Horse exercise was a great remedy with Sydenham.

Apart from what is described by him in the "*Processus*," there are endless other teachings of Sydenham which, when perused, awake attention. Some would, however, be thought objectionable—the following especially so, although he prides himself upon it. In cases of great exhaustion, when other means have failed him, he has made use of "a singular expedient with great success, namely, the application of the heat of strong and healthy

young men. I have never found," he continues, "the repeated application of warm napkins to prove near so serviceable as the present method, where the heat applied is not only more natural to the human body, but also more mild, moist, equable, and constant. Nor do I think it below me to have mentioned this expedient, whatever censure it may expose me to from such as contemn whatever is vulgar, since the health and benefit of mankind ought to be preferred to their false opinions of things."

Some idea of the mode of prescription by Sydenham is supplied by the annexed unique letter which, through the kindness of my learned brother Dr. Munk, I am able to publish for the first time in facsimile. It was a letter evidently preserved by his son William Sydenham, since it is docketed at the back, "*Letter belonging to my Father.*" The letter paper is good, stout, well-made stuff, with an inch and a quarter margin on the left-hand side.

Enough has now been said to illustrate the views of Sydenham in regard to therapeutics. He lived before the time when the iatro-mathematicians flourished, and was therefore untouched by the mechanical philosophy. He lived at a time when the chemical ideas of Paracelsus were losing their hold. He practised, therefore, rather an empirical doctrine based on simple observation; yet he also held by a zymotic theory like that we now accept. Fevers he held to be of fermentative origin, the symptoms of them arising from one of two causes—the excitement of the fermentation, or the influence of products of false fermentation acting as poisonous products. Once more we have brought before us a line of thought which in this day has been revived and passed for new.

I should be verry glad if it might be that
your childes fitt in regard of its tenderness of age
would go away of its owne accord, to which pur-
=pose were it my owne I think I should
put the cure upon giving it now and then
a large spoonfull of sack morning & two a clock
in the afternoon and at night, but in case
upon tryall the fitts shall grow still worse
so as to endanger her (for the avoiding of
which pray God her not be confined to the
bed or chamber) then I desire you to use
the Barks after this manner R. cortic.
Peruv: subtilis: pulv: ℥ij ℈ij: ds rotul.
g.s. f: elect in ℥ij portio aq: dividend:
cap.t unam. mane vel quarta quaq: horâ
solutam in cochl: j. Julap: ros: simplic: ℥iiid
cochl: ij. R aq: cinnam: corator: mgr: ℥vj cinnamo
fort: ℥vj: syrup: de myrtill: ℥vj. M. you know
that the first dose is to be taken just at
the going off of the fitt: this with my
humble service to y.r Lady is all at present
from

y.r most humble & obliged Serv.t
The Sydenham

P.S. I cannot be obliged to any spot, but
shall wish your childe as good health
as at other times.

To Sydenham has generally been accredited the introduction of Jesuits' Bark (Cinchona) into England as a cure for agues. In this there is some error. Sydenham played a considerable part in the introduction, and to his judgment, liberality, and learning we are, perhaps, most of all indebted. But the historian must be just, and although he may not like the character of the man, he is bound to give to Sir Robert Talbor the credit of first using, successfully and confidently, Peruvian Bark for the cure of agues. Him Sydenham followed. Talbor was in full swing of practice, with bark as his sheet anchor, in 1666, while Sydenham yet tried it and doubted. Afterwards, without any of the secrecy of his cotemporary, he became more confident; and, finally, he became the one authority whom all relied on.

Again, Sydenham has been credited as the author of the "cooling method of treatment in small-pox." Here also a rival has been found in Dr. John Crane, who in a preceding generation instituted the plan. The claim is fairly founded, but it is due to say that, about 1682, the honour of placing the "cooling regimen" distinctly before the world belongs to Sydenham.

IN EPITOME.

A book has yet to be written on Sydenham. The friend of Locke and Boyle, he ranks in some measure with them. His latinity has been questioned, and Dr. Mapletoft and Mr. Gilbert Havers of Cambridge have been named as translators of his works from English into Latin. I know not how far this may be true; it is not unreasonable, and it is of little moment.

I should not, for my part, call Sydenham the in-

ventor of any new system in medicine. He merely, in the midst of much controversy, held the old ground, stood by the father and fathers of physic,—a medical puritan in scientific as well as political faith.

The last years of Sydenham were marred by physical pain. He began early in life to suffer from gout. In his fifty-second year — in 1675-6 — he was affected severely by it, and he never ceased to be actually free from it from that time onward. Later his symptoms of gout became complicated with those of stone in the bladder and hæmaturia. In these his later days he lived through a martyrdom; but, says Wallis, "he never betrayed any indecent impatience or unmanly dejection under his torments, and in every interval of ease he applied himself to the assistance of others with his usual assiduity. So well did he bear up that on May 17th, 1676, he proceeded Doctor of Medicine as a member of Pembroke College, Cambridge; and although he must have been wealthy, he continued in practice so long as he could be useful. On the 29th of December, 1689, at the age of sixty-five years, he died in his house in Pall Mall, and was buried in St. James's Church, where a tablet, erected by the College of Physicians in 1810, records in elegant Latin some of his many virtues—*lubentissimè*."

ORIGINAL RESEARCH, EXPERIMENTAL AND INDUCTIVE.

"Dire n'est rien ; faire est tout."—RENAN.

ON DEHYDRATION AND MOULDING OF ORGANIC STRUCTURES UNDER WATER GAS.

IN the last two essays of original research, published in the ASCLEPIADS of the second and third quarters of the present year, I have treated on Vital Hydration and on Vital Cohesion. In the essay now in hand I am about to describe some experiments by which I once attempted to destroy or rather transmute organic parts and structures by super-hydration at a high temperature. The results were as unexpected as they were curious, and will, I think, attract the attention of many of my readers, not only because of their physiological, but, strange to say, their geological, character.

I used for my purpose the common vulcanising apparatus of the dentist depicted in the diagram (Fig. 1). It consists of a strong chamber of iron enclosed in an iron case or stove, with a series of gas-burners at the lower part of the stove. The iron chamber, which receives the substances to be operated upon, was heated by the burners. It was furnished with a heavy iron lid with binding-screws, a safety-valve, and a tube for holding mercury, in which a thermometer was inserted.

The chamber was ten inches deep and five inches in diameter. Six to eight fluid ounces of water were usually placed in the chamber for one series of experiments, but the quantity was varied, by which different results were obtained.

The specimens of animal structures to be experimented on were introduced into the chamber in different

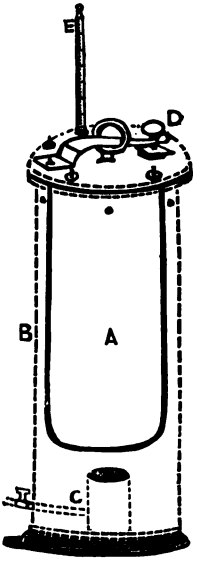

FIG. 1.—*a*, iron cylinder; *b*, stove; *c*, gas burner; *d*, lid with safety-valve; *e*, thermometer.

ways. In some cases I placed the specimen directly in the chamber in or above the water; in other cases I put it in an iron flask filled with wet plaster of Paris, lime, carbonate of lime, powdered carbon, clay, powdered Portland stone, or other substance, and subjected the whole to pressure by compressing screws. I constructed also a convenient iron flask for this purpose. It consisted of a framework of iron, with two plates of

iron to make a false top and bottom. The frame laid on the lower plate formed a flask, and into it the plaster of Paris, or clay, or sand, or carbon, moistened with water, was placed, with the specimen embedded. Then the upper plate of iron was dropped on, an encircling band of iron was passed over the whole lengthways, two screws in this band were brought forcibly down on the

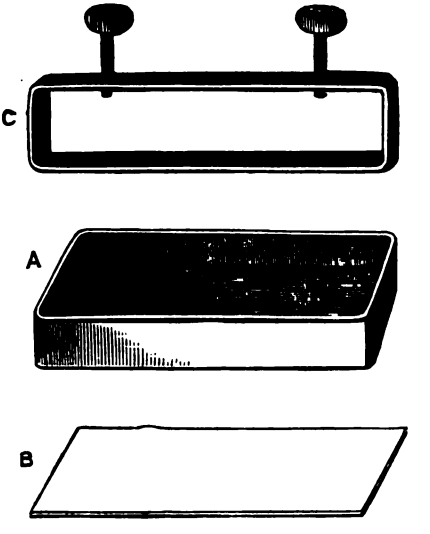

FIG. 2.—*a*, flask ready for filling ; *b*, loose lid ; *c*, encircling band, with compressing screws.

upper plate of the box, and thus the specimen, with the substance in which it was buried, was firmly encased. The iron flask, in this way arranged, was now ready to be placed in the vulcaniser. The advantage of this flask was that when the exposure to heat was completed and the metal cooled down, on setting free the iron band the false top and bottom could be removed, and the specimen cut out with a small keyhole saw from its

iron framework. The flask is depicted in the diagrams (Fig. 2) with its several parts.

Having placed my specimens and the water in the iron chamber, I screwed on the lid firmly, interposing what may be called a washer of brown paper between the lid and the chamber at the part where they touch; I screwed down the safety-valve, interposing between it and the small opening it covers also a layer of brown paper; I put the thermometer into the mercury, lighted the gas, and watched the rise in the thermometer up to the point of heat required. The necessary degree of heat obtained, the gas was turned a little down and moderated until the mercury remained steadily at one point, and the experiment was continued for whatever length of time was desired.

With these preliminaries, I pass to describe some of the results which were obtained.

Dehydration Blood.

Into the chamber of the apparatus a portion of blood-clot, from the blood of an ox, was placed on a shelf with eight ounces of water beneath. The lid of the chamber was firmly adjusted. The heat was raised to 340° Fahr., and was sustained at that degree for one hour and a half. The heat was then withdrawn, and some hours were allowed for cooling. On opening the iron chamber the blood was found almost unaltered in shape, but altogether changed in consistence and structure. It felt like simple caoutchouc, but broke with a bright surface like Spanish liquorice. The natural characteristics of the blood were lost, and on gentle drying the mass became brittle, closely resembling jet. A specimen of blood thus treated was examined

by my friend Dr. Leonard Sedgwick, who reported upon it that it "was a bright black, friable, jet-like material. "Gently rubbed down with a little distilled water, it formed a reddish-brown fluid, which under the microscope was seen to consist of a coloured liquid and reddish granular masses of various sizes. Very many were about one-sixth the size of a blood-corpuscle, reddish-brown, and very irregular in shape. As the solution dried one or two irregularly hexagonal crystals made their appearance. The substance, after twenty-four hours' soaking, was partially soluble in strong solution of ammonia, very slightly in distilled water, and hardly at all in dilute hydrochloric acid, or in methylic alcohol; it was untouched in ethylic ether and in chloroform.

Into the iron box or flask plaster of Paris was poured in the fluid state, and a clot of fresh blood was immersed in the plaster. The lid was placed on the box, and when the plaster had set firmly, the whole was placed in the chamber with six ounces of water. The temperature was raised to 340°, and sustained for an hour and a half. On breaking up the plaster, after cooling, the blood was found in the same state as that named in the experiment described above.

Dehydration of Albumen.

An egg in the fluid condition was placed in the iron flask and surrounded with plaster of Paris. When the plaster was entirely set the flask was put into the chamber with six ounces of water; the temperature was raised to 340°, and was sustained for an hour and a half. After cooling, which was very rapidly effected by immersing the flask in cold water, I found, on removing

the egg, that the shell was nearly full of a beautiful transparent golden or amber-coloured fluid, very thin, and running like dissolved gelatine. In the course of a few hours this fluid was slightly gelatinised. The membrane lining the shell was detached, but not destroyed; the shell was dry, brittle, and firmly attached to the surrounding plaster. The experiment was repeated with another egg, but was modified by allowing the apparatus to cool very slowly in the air at 60°. On breaking the plaster and cutting through the egg, no fluid was found, but in the centre a soft yellow substance (probably the yolk), about the size of a hazel nut, and slightly glistening on the surface. On gently drying this substance it became firm, retaining its colour and looking like amber, but not so hard.

THE BODY OF A DEAD TOAD DEHYDRATED IN CARBON.

The iron flask was partly filled with fluid plaster of Paris. On this layer a bed of vegetable carbon, in fine powder, was laid, and the body of a toad recently dead was buried in it. The carbon mound was next enclosed in plaster; the flask was closed, and half an hour later it was placed in the iron chamber with ten ounces of water. The temperature was first raised to 350° Fahr., but was brought down to 340°, and was retained at this degree for an hour and forty minutes. The gas was then turned off, and the apparatus was allowed to cool slowly. On opening the flask the body of the animal was found to be altogether destroyed, and so mixed with the carbon that no part of it could be defined.

DEHYDRATION OF THE BODY OF A DEAD FROG IN SAND.

The body of a frog recently dead was buried within the iron flask, in moist fine sand compressed with

moderate firmness. The flask was then put into the iron chamber, with six ounces of water, and the temperature was raised to 340° Fahr., and sustained for an hour and a half. The flask was opened twelve hours afterwards, and the results of the experiment were found to be nearly the same as when carbon was employed. The animal was destroyed, and no distinct organ or structure could be distinguished.

Dehydration of the Body of a Dead Frog in Plaster of Paris.

Fluid plaster of Paris was poured into the iron flask until the flask was half full. The body of a frog recently dead was now laid on the plaster, and allowed to mould itself to it. When the plaster had become rather firm, another quantity of fluid plaster was poured in, so as to bury the frog completely and fill the flask. An hour later the flask, which had been closed, with pressure, was placed in the iron chamber. The temperature was raised to 340° Fahr., and sustained for two hours. Twelve hours later the flask was opened, and a mould of the frog was found, the organic soft parts of the body having been destroyed. At the lower part, in the centre, was a black spot; the spot consisted of blood which had gravitated to the lowest part. Besides this, there was a little *débris* of earthy part of bone within the mould. The impression of the body was beautifully marked in the plaster.

Dehydration of the Body of a Fish in Plaster and Alum.

Some plaster of Paris, made into a fluid with water containing alum in solution, was poured into an iron

flask until the flask was half filled. The body of a dead fish, a common sprat, was cut in half transversely, the two halves were laid upon the plaster, and the flask was filled up with fluid plaster and closed. When the plaster was firm the flask was placed in the iron chamber, with four ounces of water, and the temperature, raised to 340°, was sustained at that degree for an hour. Twelve hours afterwards the flask was laid open, and the plaster cut in half, when two moulds were found: one of the upper, the other of the lower, half of the fish. The markings of the body of the fish were delineated on the mould; a small portion of bone (spinal) was left; a dark-coloured fine spot, surrounded by a shiny scaly substance, indicated the position of the eyeball; a little filamentous *débris* remained, consisting probably of the scaly covering of the animal.

After exposing the body of a dead fish to heat in plaster of Paris as described in the last experiment, I made two comparative experiments. In the first of these I placed another similar fish above the water in the iron chamber simply, not encasing it, that is to say, in any substance. The heat was then raised to 340° Fahr. and sustained for an hour and a half. Twelve hours afterwards, on opening the chamber, the fish was found to have been entirely destroyed, with the exception of some small portions of the scaly covering. In the second comparative experiment, by means of a rod, I plunged the body of a dead fish suddenly under molten lead. The whole of the structure of the animal was immediately reduced to a charred mass, the carbon alone remaining undestroyed. I noticed that a portion of the charred mass, spongy in character and light, retained the form of the animal as it floated to the surface of the lead, from which

I saw that it was quite possible to obtain what might be called a carbon skeleton of an animal—a skeleton, that is, of the base on which all the organic structures are formed during life. From the first of these two experiments it is clear that exposure of an animal body direct to water gas at a high temperature is rapidly destructive. From the second experiment we learn that the sudden exposure of an animal to intense heat without moisture destroys by a different or, at all events, by a modified process. This may be due to the rapidity of action of the greater heat, the lead while in the molten state having a temperature of 604° Fahr.

Dehydration of the Eyeball in Plaster of Paris.

An eyeball removed from an ox recently killed was placed in the iron flask surrounded by plaster of Paris in the fluid state. When the plaster had set the flask was placed in the iron chamber with six ounces of water, and the heat, raised to 340° Fahr., was sustained at that degree for an hour and a half. Twelve hours afterwards, on opening the flask, a very perfect mould of the eyeball was found in the plaster, but all the structures were resolved and removed except the pigment. At the bottom of the mould there lay a dark thin membranous substance, perforated in the centre, and being, I think, the posterior part of the iris, with a portion of the choroid.

Dehydration of Sponge in Plaster of Paris.

A portion of fresh clean sponge was placed in the iron flask, and was surrounded with plaster of Paris in the fluid state. The flask was closed with gentle pressure, and when the plaster it contained was firmly set it was put into the iron chamber with six ounces of water.

The heat was raised to 340°, and was sustained at that degree for two hours. The apparatus was then allowed to cool, the flask was removed and opened. On breaking across the plaster of Paris block, a beautiful cast of the structure of the sponge was presented. The appearance of the cast is attempted to be depicted below in Fig. 3. The sponge was not entirely destroyed,

FIG. 3.

there being left in the mould a sort of fine webwork, which stretched from one raised point to another. This webwork resembled spider's web in lightness and structure, and it absorbed water from the air.

DEHYDRATION OF THE BODY OF A DEAD FROG IN CHALK.

The iron flask was filled with finely powdered chalk in which the body of a frog recently dead was buried.

Water was now poured on the chalk until it was brought to a soft mass; then the flask was placed in the iron chamber with six ounces of water, and for the space of an hour and a half was exposed to a heat of 340° Fahr. On opening the flask after cooling, the chalk was found nearly as firm as it is in natural cliffs. The body of the frog was nearly all destroyed, but a flattened mould of it remained. The markings were less clearly defined than in previous specimens.

Dehydration of the Body of a Dead Fish in Portland Stone.

A portion of Portland stone was reduced to a fine powder, and was poured into the iron flask; a fish was buried in the powder, and water was added until the mass was made of the consistency of good mortar. The upper lid of the flask was then put on, the binding screws were applied so as to produce firm pressure, and the whole was placed in the iron chamber with six ounces of water. The heat was raised to 340° and sustained at that degree for one hour and a half. Next day the flask was opened, and the powdered stone was found a solid mass. The body of the fish had been destroyed, but there remained a most perfect marking of the body. The mouldings of the animal in this case were shallow—in fact, almost flat—and as the block dried they crumbled rapidly away.

Dehydration of the Bodies of Dead Animals in Silicates, with Lime.

Mr. Hockin, of Duke Street, supplied me with a solution of silicate of soda and potassa, which he had prepared for the purpose of hardening stone. This

solution, made into a paste with carbonate of lime, produces a very firm and even stone. I made a paste of this kind, and filled the iron flask with it, interposing in layers several dead animal substances, viz., two dead fishes, an oyster, two cockles, some prawns, and a piece of the lung of a sheep. While the paste was still soft the lid of the flask was put on, and very firm pressure was applied with the compressing screws. The flask

FIG. 4.

was then dropped into the iron chamber, with six ounces of water, and the temperature, raised to 340° Fahr., was kept up for two hours. Twelve hours later the flask, which had cooled to the temperature of the day, was opened, and was found to enclose a fine solid stone. The stone was cut out and broken with a hammer. The piece of lung was destroyed altogether, but the markings of its structure were delineated, and I afterwards took advantage of the observation to get a

perfect mould of the interior surface of the lung. The organic structures of all the other animals were removed, but the outer hard coverings of the prawns, cockles, and oysters remained so embedded and blended with the stone that they seemed to form a part of it. The block, indeed, would easily pass for a natural product amongst the large majority of people; and it suggests the idea of the manufacture of artificial fossil remains. In the drawing above (Fig. 4) the artist has endeavoured to show the appearance of a fragment of this artificial stone.

The Leaf of a Plant Dehydrated in Carbon.

The iron flask was filled with finely powdered carbon; in the middle were placed a bunch of parsley, the stem of a leaf, and a leaf itself. The carbon was wetted, and then firmly compressed in the flask. The flask was next placed in the iron chamber, with six ounces of water, and the heat, raised to 340°, was maintained at that degree for two hours. After cooling the flask was opened, and on breaking across the block of carbon a beautiful tracing of the leaf was found. A portion of the stem remained in a filamentous form.

Mouldings of Dead Animals.

It is worthy of notice in respect to the mouldings of animals left in substances in a state of semi-fluidity, that a moulding may sometimes be obtained in relief. This occurs from position and from pressure, at a certain degree of heat, in the process of hardening of the surrounding material. In one experiment a dead frog was placed in plaster of Paris within a flask, and while the plaster was wet the lid of the flask was firmly screwed down upon the plaster. The upper part of the mould

of this animal was found (after the exposure to water gas at 340° for two hours) so distinctly that the markings stood out boldly, giving an idea of parts of the body in relief. The imitation was precisely like what is often seen in stone containing fossil remains.

Dehydration of Albumen with Phosphorus.

An egg was embedded within the iron flask in plaster of Paris, except for a small space on the upper part. An opening was drilled through the shell, and into the opening one grain of pure phosphorus was passed, and was pushed with a thin glass rod into the centre of the yolk of the egg. The opening into the egg was covered, and the flask was filled with the plaster. After the plaster was well set the whole was placed in the iron chamber, with six ounces of water; and the temperature was raised to 340° Fahr., which temperature was kept up for two hours. When the flask was opened some hours afterwards, and the plaster was cut through to the egg, the shell of the egg was found to contain a brownish red-coloured paste, looking much like the marrow of bone. When the smallest part of this paste was rubbed, there was combustion of phosphorus, showing that this element, although so finely diffused through the mass, was in the free state. The paste was partly soluble in ether, in alcohol, and in water.

Dehydration of Albumen with Sulphur.

An egg was placed in plaster of Paris, as in last experiment. The shell of the egg was perforated, and into the centre three grains of sulphur were passed in small fragments. The opening in the egg was closed, and the flask, filled up with the plaster, was put into

the iron chamber, after the plaster was set, with six ounces of water. The heat was raised to 340° Fahr., and kept up to that degree for two hours. Some hours later, the apparatus having cooled, the flask was removed and opened. The albumen was found in the form of a paste of brown colour and moderately firm consistency.

Dehydration of Blood with Sulphur.

Some clot of blood was mixed with sulphur, in proportion of fifty parts of blood to one of sulphur. The mixture was placed in a porcelain crucible, and being put into the iron chamber over six ounces of water, was exposed for two hours to a temperature of 340° in the water gas. On removing the clot, after cooling, it was found in the same condition as the blood that had been subjected to the same degree of heat without sulphur—*i.e.*, it was firm and felt like caoutchouc, but broke with a fracture having a bright surface. In a second experiment, blood clot and sulphur were enclosed in plaster of Paris within an iron flask, and were subjected, with six ounces of water, to heat at 340° for two hours. The result was the same as in last experiment.

Dehydration of a Portion of an Artery with Sulphur.

A piece of artery, from the aorta of an ox, measuring one inch long and one inch wide, was laid on a bed of moist plaster of Paris within a small iron flask. Beneath and above the piece of artery was placed a thin layer of finely powdered sulphur. The flask was filled with plaster, closed, and, with six

ounces of water, was put into the iron chamber, where for an hour and a half it was exposed to a heat of 340°. On opening the flask, after cooling, the piece of artery was found, reduced in size and not thicker in structure than tissue paper; it was elastic, and so like a portion of very fine dark india-rubber sheeting, it would easily have been mistaken for that substance. In a few hours it grew hard, and broke with a bright, almost glassy surface, like vulcanite base.

COMMENTARY.

It will be seen that in these experiments I have recorded only the results obtained at a temperature of 340° Fahr., extended over a period of time ranging from one hour and a half to two hours. What differences would occur from exposure at higher temperatures over longer periods remain yet to be determined.

From what has already been done we may learn, however, something that is of interest, and, indeed, I doubt not that, were the inclination indulged, a great deal of curious speculative argument of analogical character might be put forward. I have no intention myself of gratifying such inclination, and shall confine the comments I make to narrow bounds.

Let me say, then, as a first commentary on what I have seen, that the process of producing changes in organic animal substances by the means pointed out in this paper is not likely to be given up if it be once fairly tried by the physical inquirer.

It will be observed from my experiments that on exposing some animal substances, especially blood, to so high a degree of heat as 340° in water gas, there is no destruction of matter, but very distinct change of

character. It will also be observed that the pigmentary matter of the eyeball that was exposed to the same high degree of heat was not destroyed. It will be further observed that when the body of the dead frog buried in plaster of Paris was disintegrated by the heat a dark substance, evidently blood, remained. We see, I think, in these facts the persistent character of the pigmentary structures of animal substances. These seem not merely to be themselves resistant, but, in the case of blood, to hold together other substances with which they are immediately surrounded.

After the blood the bones seem most resistant, and, in the case of fish, the scaly covering of the body. The shells of the oyster, the cockle, and periwinkle are resistant at 340°, but the enclosed animal substance is destroyed. The outer covering of the prawn is resistant.

It will be gathered further from these experiments that animal bodies buried in moist substances and exposed to a heat of 340° Fahr., although almost destroyed, leave more or less evidence of the fact that they have been in existence, the evidence varying according to the character of the bed in which the destruction has taken place. Destruction in vegetable carbon is extremely rapid and complete in respect to all the soft structures; destruction of the same in sand, in carbonate of lime, and in sulphate of lime, is also rapid.

Under certain circumstances this destruction of animal matter while embedded in moist substance is attended with the formation of a cast or mould of the animal, with or without the remaining portions of the more resistant structures. This was well marked in the dead fish that was encased in plaster of Paris

and alum, the body of the animal was destroyed, but a good mould was left, as shown in the next sketch (Fig. 5).

The soft and most highly organised structures of animals are, it is clear, readily disintegrated in water vapour at the temperature of 340° Fahr.; the pigmentary matters and also those structures of animals which contain most earthy or inorganic substance are not,

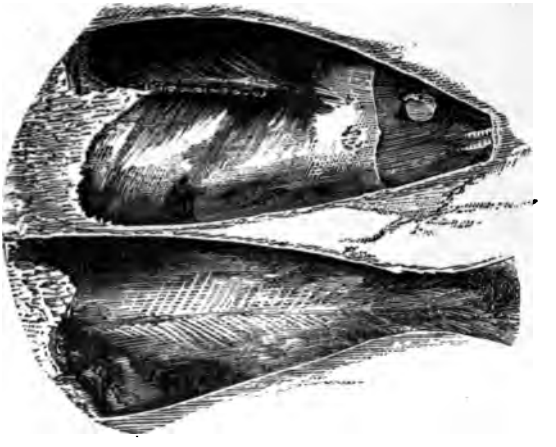

FIG. 5.

however, so easily resolvable, but have a tendency, in some instances, to combine mechanically with the matter in which they are embedded. In these results we see much that resembles what is found in the earth as indication of animal remains; for in the earth we find casts of animal bodies, outer coverings of animal bodies embedded in stone, and bony structures fixed in stone. But to make the analogy of the artificial and natural perfect, we require one other factor in our research—I mean continued and intense pressure while

the process of disintegration of the soft structure of the body is in progress. Then the surrounding material while in the moist state would be forced into the space previously filled by the easily destructible parts of the animal, while the less destructible parts of the animal, such as the skeleton or the horny covering, would become, as it were, a portion of the concrete, and would undergo subsequent solidification with the concrete.

In relation to this seeming analogy—and I am not prepared to refer to it as anything more at this moment —the most singular fact is that relating to the rapidity with which, in experiment, the changes are effected. What is done in the iron chamber charged with water gas at 340° Fahr. in the course of from one to two hours might, if the facts were not known to us, pass for the work of a vast period of time. At this point the argument from analogy of the artificial with the natural must be suspended until experiments at a lower temperature have been carried out. We are, in fact, limited at this moment to one inference—viz., that if, by any commotion or other physical accident, a bed of loose earth, covered with living animals, were suddenly to subside under water, so as to bury all the living things in it; and if, from motion or other cause, the temperature of the mass were raised to 340° Fahr. for the space of from one to two hours, the truly organised parts of all the animals embedded would be removed, and the indications of the animals having existed would be left either as mere moulds, or as incrustations, or as animal remains from which the organic substance had been removed by the heat and had been replaced by the surrounding inorganic matter which formed the bed.

COTEMPORARY PRACTICE AND LITERATURE.

"Every physician will and ought to make observations from his own experience; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together."—FRIEND.

OPERÆ PRETIUM EST.

THE PHYSIOLOGY AND PREVENTION OF INFLUENZA.

DR. JULIUS ALTHAUS' paper on Influenza, read originally before the Medical Society of London, has now expanded into a volume of 394 pages (Longmans & Co.), and has reached a second edition. This fact alone testifies how fully the work has been appreciated by the profession of medicine, to whom alone it appeals. The work is made up of six chapters, the second, third, and fifth of which, embracing the subjects of "the feverish attack of the disease, the complications and sequels, and the prognosis, afford most excellent reading, and show on every page the accomplished and careful observer and physician. The other three chapters, all of which naturally hang together, will be considered valuable to the many who hold to the germ hypothesis as if it were gospel; but to those who are not so sure about the foundations of the germ

hypothesis they are not so satisfactory. How is it possible to accept the hypothesis on page 13, that "the patient having acquired infection, a poisonous albuminoid secreted by the bacillus of grip, and which I call the *grippo-toxine*, circulates in the blood and causes the special symptoms of the feverish attack." This is to me a mere speculative fancy, that would ruin the book altogether but for the admirably drawn descriptions, from nature, of the complications and sequelæ, especially those relating to diseases affecting the nervous system, of which the author is so competent a teacher.

THE NATIONALISATION OF HEALTH.

MR. HAVELOCK ELLIS, whose work entitled "The Criminal" has been noticed in these pages, now presents to us (Fisher Unwin & Co.) a neat volume on the subject named above. Mr. Ellis always writes with a purpose, and a good one too; and in the present volume he deals with a great subject in an able manner. Commencing with the text that "the fate of man is in his own hands," he proceeds to argue that the time has come "for taking in hand seriously the organization and socialization of the elementary conditions of health." The argument throughout is full of interest, and may be recommended very earnestly to the faculty of physic. There are points in it, it is true, on which it is impossible for all to agree with him, especially one in which he compares the present system of medical education conducted in the hospital and the school with the old and splendid system of preliminary private pupilage; but his ideas are broad,

his objects excellent, and his originality conspicuous on many and difficult subjects of the suggestive order.

MYTHS AND DREAMS.

TO the comparative few who have the delight of knowing him personally, Edward Clodd is indeed a welcome visitor. To the thousands who know him as a book, he is equally welcome; and the second edition of his "Myths and Dreams" (Chatto and Windus) will be received with certain favour of the right sort. "The object of the book is," says the author, "to present in compendious form the evidence which myths and dreams supply as to primitive man's interpretation of his own nature and of the external world, and more especially to indicate how such evidence carries within itself the history of the origin and growth of beliefs in the supernatural." I leave those who have not had the treat of reading the book to find out for themselves how far its author has justified his intentions.

TO CORRESPONDENTS.

In order to prevent the frequent and troublesome mistake of addressing communications to the "Editor," it is necessary to repeat that everything published in the ASCLEPIAD *is exclusively by the Author whose name is on the title-page.*

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The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

December 15th, 1892.

APPENDIX OF ACKNOWLEDGMENTS.

ALCOCK. "Association Football." By C. W. ALCOCK. London. 1892.

ALLCHIN. "The Nature and Causes of Duodenal Indigestion." By W. H. ALLCHIN, M.B. London: John Bale & Sons. 1892.

ALTHAUS. "The Pathology and Prevention of Influenza." By JULIUS ALTHAUS, M.D. London: Longmans & Co. 1892.

ANDERSON. "The Antiseptic Treatment of Typhoid Fever." By A. M. ANDERSON, M.D. Dundee: J. Leng & Co. 1892.

ASHBY & WRIGHT. "The Diseases of Children." By H. ASHBY, M.D. and G. A. WRIGHT, M.B., F.R.C.S. London: Longmans & Co.

BARTON. "Tables for the Diagnosis and Treatment of Syphilis." By JOHN KELLOCK BARTON. Dublin: Fannin & Co. London: Baillière & Co. 1888.

BATEMAN. "On Aphasia." By SIR FREDERIC BATEMAN, M.D. London: J. & A. Churchill. 1890.

BELL. "Medico-Legal Studies." By CLARK BELL, Esq. New York. 1891.

BENNETT. "A Sketch History of Marlborough in Neolithic Times." By F. J. BENNETT, F.G.S. Marlborough: G. Gale. 1892.

BLACKWELL. "Erroneous Methods in Medical Education. Why Hygienic Congresses Fail." By ELIZABETH BLACKWELL, M.D. London: G. Bell & Sons. 1892.

BLAKE. "On the Actions of Sepsin, of Lead, and allied agents." By EDWARD BLAKE, M.D. London: Straker Bros. & Co. 1892.

BLEYER. "The Edison Phonograph and the Bettini Micro-Phonograph." By J. MOUNT BLEYER, M.D. New York. 1892.

BOUFFÉ. "Nature et Traitement de la Diphtérie." Par FLORIS BOUFFÉ. Clermont (Oise) Imprimerie: Daix Frères. 1889.

BRIDGES. "Harvey and his Successors." Harveian Oration for 1892. By J. H. BRIDGES, M.B. London: Macmillan & Co. 1892.

BRODIE. "Dissections Illustrated." By C. GORDON BRODIE, F.R.C.S., with plates drawn and lithographed by Percy Highly. London: Whittaker & Co. 1892.

CACHEUX. "État des Habitations Ouvrières." By Emile Cacheux. Paris: Baudry & Co. 1891.

CAMPBELL. "Biological Teaching in the Colleges of the United States." By J. P. CAMPBELL, Ph.D. Washington: Government Printing Office. 1891.

CAMPBELL. Bureau of Education. Circular of Information No. 9: "Biological Teaching." By JOHN P. CAMPBELL, A.B., Ph.D. Washington: Government Printing Office. 1891.

CATHELL. "Book on the Physician Himself." By D. W. CATHELL, M.D. Philadelphia and London: F. Davis Co. 1892.

CHADWICK. "Temperament, Disease, and Health." By FRENCH ENSOR CHADWICK, U.S.M. New York and London: G. W. Putnam's Sons. 1892.

CHRISTY. "New Commercial Plants and Drugs." By T. CHRISTY, F.L.S. London: Christy & Co. 1885.

CLARK. "The Annual Address." Delivered to the Royal College of Physicians, April 1892. By SIR ANDREW CLARK, M.D., F.R.S. London: John Bale & Sons. 1892.

COBB. "Earth-burial and Cremation." By AUGUSTUS G. COBB. New York and London: G. P. Putnam's Sons. 1892.

COOK. "On Asepsia." By EDMUND ALLEYNE COOK, L.R.C.P. Richmond. 1892.

COOKE. "Tablets of Anatomy." By THOMAS COOKE, F.R.C.S., B.A., B.Sc., M.D. London: Longmans, Green & Co. 1892.

CORMACK. (1) "Vichy." (2) "Hyères as a Health Resort," *Climatologist*, 1891. By CH. E. CORMACK, M.D.

COSGRAVE. "The Control of Inebriates." By MACDOWEL COSGRAVE, M.D. Dublin: Fannin & Co. 1892.

COX. "Hastings and St. Leonards." By F. AUGUSTUS COX, M.B. Manchester and London: John Heywood. 1892.

CUTTER. "Heart-rest Sanatory." By EPHRAIM CUTTER, M.D. New York and Boston. 1892.

DAVIS. "Brief Comments on the Pathology of Pneumonia." By N. S. DAVIS, M.D. Chicago. 1892.

DAVIS. "Diseases of the Lungs, Heart, and Kidneys." By N. S. DAVIS, junr., M.D. Philadelphia and London: Davis Co. 1892.

DIXON. "Reaction of the Amide Group upon Wasting Animal Economy." By PROFS. SAMUEL D. DIXON, M.D., and W. S. ZERILL, M.D. Philadelphia: American Medical Press Company. 1891.

DUDFIELD. "The Annual Report on the Health, Sanitary Condition, etc., of the Pariah of St. Mary Abbotts, Kensington, for 1891. By T. ORME DUDFIELD, M.D. Kensington: S. Sidders. 1892.

EDWARDS. "Urinary Surgery." By F. SWINFORD EDWARDS, F.R.C.S. London: Bailliére, Tindall & Cox. 1892.

EINHORN. "Ueber Achylia Gastrica." Chemical Publishing Company. 1892.

——— "Therapeutic Results of Direct Electrisation of the Stomach." By VON MAX EINHORN, M.D. New York. 1892.

ELLIS. "The Nationalisation of Health." By HAVELOCK ELLIS. London: T. Fisher Unwin. 1892.

ESHNER. "Arterio-Capillary Fibrosis." By AUGUSTUS A. ESHNER, M.D. Philadelphia. 1891.

FARQUHARSON. "'Ptomaines' and other Animal Alkaloids."

——— "Auto-Toxæmia." By A. C. FARQUHARSON, M.D. Bristol: J. Wright & Co. 1892.

FELKIN. "Health Resorts of the World." By R. W. FELKIN, M.D. Edinburgh. 1892.

FOWLER. "On some Modern Aspects of the Relations between Religion and Science." By W. W. FOWLER. Lincoln: J. Williamson, 1892.

GOODHART. The Harveian Lectures for 1891. "On Common Neuroses." By JAMES F. GOODHART, M.D. London: H. K. Lewis, 136, Gower Street, 1892.

GORDY. Bureau of Education. Circular of Information No. 8: "Rise and Growth of the Normal-School Idea in the United States. By J. P. GORDY. Washington: Government Printing Office. 1891.

GRESSWELL. "A Contribution to the Natural History of Scarlatina." By D. ASTLEY GRESSWELL, M.D. Oxon. Oxford: Clarendon Press. 1890.

HEWAT. "Life Assurance." By ARCHIBALD HEWAT. London: Waterlow & Sons. 1891.

HEWITT. "Experiments on the Production of an Imitation of Sea-Sickness by Complex Visual Disturbances." By GRAILY HEWITT, M.D. London. 1892.

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HILL. "From Reflex Action to Volition." By ALEXANDER HILL, M.D. London. 1892.

HISTORICUS. "Cocoa. All about it." By HISTORICUS. London: Sampson Low, Marston & Company. 1892.

HOLLANDER. "Phrenology: The Old and the New." By BERNARD HOLLANDER. London: British Phrenological Association. 1892.

JEFFRIES. "Eye Paralysis." By JOHN AMORY JEFFRIES, M.D. Boston, U.S.A.: Darrell & Upton. 1892.

JENNINGS. "Cure of the Morphia Habit." By OSCAR JENNINGS, M.D. London: Bailliére, Tindall & Cox. 1890.

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JONES. Contributions to Orthopædic Surgery. By R. JONES, F.R.C.S.E., and J. RIDLON, M.D. London. 1892.

KINGSTON. "Intemperance: Its Causes and its Remedies." By W. BEATTY KINGSTON. London: Routledge & Sons. 1892.

KYLE. "The Pathology and Treatment of Tetanus." By D. BRADEN KYLE, M.D. Detroit, Michigan: G. S. Davis. 1892.

LAWRENCE-HAMILTON. "Imperishable Fresh Fish." By J. LAWRENCE-HAMILTON. London: P. S. King & Son. Westminster. 1892.

LORENZO. "L'Ittiolo nella cura di alcuna dermatosi." By GIACOMO DI LORENZO. Napoli. 1892.

LOWNDES. "Reasons why the Office of Coroner should be held by a Member of the Medical Profession." By FREDERICK W. LOWNDES, M.R.C.S. London: J. & A. Churchill. 1892.

LUTAND. "Etudes sur la Rage et la Méthode Pasteur." Paris. 1891.

M'INTOSH. The Scottish Fisheries. By W. C. M'INTOSH, M.D., LL.D., F.R.S. Dundee: John Long & Co. 1892.

MACDONALD. "The Infliction of the Death Penalty by Means of Electricity." By C. F. MACDONALD, M.D. New York: D. Appleton & Co. 1892.

McLAUGHLIN. "History of Higher Education in Michigan." By ANDREW C. MCLAUGHLIN. Bureau of Education. Circular of Information No. 4. 1891. Washington: Government Printing Office, 1891.

MITCHELL. Address on opening of the Institute of Hygiene. Philadelphia. 1892.

——— Address on opening the Institute of the University of Pennsylvania. Philadelphia: University of Pennsylvania Press. 1892.

——— "The Early History of Instrumental Precision in Medicine." By S. WEIR MITCHELL, M.D. Havard, New Haven. Tuttlo: Moorhouse & Taylor. 1892.

MULLEN. "Annual Report of the Hospital Department of the County Borough of Salford." By JOHN W. MULLEN, L.R.C.S. Salford: W. S. Jackson & Sons. 1892.

MUSSER. "Whooping Cough: Its Management and its Climatic Treatment."

——— "Tuberculous Ulcer of the Stomach." 1890.

——— "The Limitations and the Powers of Therapeutics." Reprint, *University Medical Magazine.* Pennsylvania. 1892.

——— "The Uses of Fever, and The Dangers of Antipyretics in Typhoid Fever." By J. H. MUSSER, M.D. From the *Medical News.* Philadelphia. 1892.

NOLAN. "Is Katatonia a Special Form of Mental Disorder?" By M. J. NOLAN, L.R.C.P. Lewes: *South Counties Press.* 1892.

O'NEILL. "Treatment of Large Arterio-Venous Nævi by Excision."

——— "Some of the Important Advances made in Surgical Practice during Recent Years." By Henry O'Neill, M.D. Belfast. 1892.

PAGET. "Annual Report of the Medical Officer of Health for Salford, 1891." By CHARLES E. PAGET, M.R.C.S. Salford. 1892.

PATTESON. "A Case of Second Infection with Syphilis." By R. GLASGOW PATTESON, M.B. Reprint from *British Journal of Dermatology,* 1892.

PETERSON. "Progress in the Cure and Colonisation of Epileptics;" "Wintering in Egypt": New York, 1892; and "Outline of a Plan for an Epileptic Colony." By FREDERICK PETERSON, M.D. Reprints. 1892.

POLYTECHNIC. "The Social Centres of London." The Polytechnic Reception Bureau. London, 1892.

RANDALL. "International Prison Congress, St. Petersburgh." By C. D. RANDALL. Washington: Government Printing Office. 1891.

RAVEN. "Diphtheria or Tonsillitis." By THOMAS F. RAVEN, L.R.C.P. 1892.

RIDGE. "Alcohol and Public Health." By J. J. RIDGE, M.D. London: H. K. Lewis. 1892.

ROBERTS. "Uric Acid, Gravel, and Gout." "Croonian Lectures" for 1892. By SIR WILLIAM ROBERTS, M.D., F.R.S. London: Smith, Elder & Co. 1892.

ROUNTREE. "A Paper on Temperance Legislation." By JOSEPH ROUNTREE. York. 1892.

SAVILL. "On an Epidemic Skin Disease resembling Eczema and Pityriasis Rubra." By THOMAS D. SAVILL, M.D. London: H. K. Lewis. 1892.

SCHOFIELD. "Friedenheim, A Home of Peace for the Dying." By ALFRED T. SCHOFIELD. London. 1892.

SINCLAIR. "Funeral Reform." By the VEN. WILLIAM MACDONALD SINCLAIR, D.D. London: *Church of England Pulpit* Office. 1892.

SMITH. "School Hygiene." By P. CALDWELL SMITH, M.D. Glasgow: A Macdougall. 1892.

——— "Sub-Peritoneal Hysterectomy." By HEYWOOD SMITH, M.D. Oxon. London: John Bale & Sons. 1892.

SOLIS-COHEN. "The Symptoms and Pathological Changes in the Upper Air Passages in Influenza." New York: D. Appleton & Co. 1892.

——— "Laryngology." A case of Tracheotomy for Multiple Neoplasms of the Larynx. By J. SOLIS COHEN, M.D. 1892.

STEARNS. "Temperance Shot and Shell." By J. N. STEARNS. New York: National Temperance Publication House. 1892.

STEAVENSON. "Medical Electricity." By W. E. STEAVENSON, M.D., and H. L. JONES, M.A., M.D. London: H. K. Lewis. 1892.

SUTHERLAND. "New and Old Ways to the Far East." By SIR THOMAS SUTHERLAND, M.P. London. 1892.

SYKES. "Public Health Problems." By JOHN F. J. SYKES, M.B. London: W. Scott. 1892.

SYMPSON. "Salol in the Treatment of Chronic Cystitis."

——— "The Causes of Death in Diphtheria, with Notes on Treatment."

——— "Salicylate of Sodium in the Treatment of Diabetes." By E. MANSEL SYMPSON, M.D. Cantab. Lincoln. 1892.

TADLOCK. "Bohemian Days." By CLARA MOYSE TADLOCK. New York: J. B. Alden. 1889.

TALLERMAN. "Farm Produce Realization." By D. TALLERMAN. London: Simpkin, Marshall & Co. 1892.

THOLOZAN. "Lieux d'Origine ou d'Émergence des grandes Epidémies Cholériques." Par M. J. D. THOLOZAN. Gautier: Villars el fils. Paris. 1891.

TUCKEY. "The Value of Hypnotism in Chronic Alcoholism." By CHARLES LLOYD TUCKEY, M.D. London: J. & A. Churchill. 1892.

TUKE. "History of the Insane." London: Kegan Paul, Trench & Co. 1882.

——— "Report on the Treatment of the Insane." By HACK TUKE, M.D. London: J. & A. Churchill. 1892.

TUKE AND PATON. "Reports from the Laboratory of the Royal College of Physicians." Edited by J. BATTY TUKE, M.D., and D. NOEL PATON, M.D. Edinburgh and London: Young & Pentland. 1892.

USHER. "Alcoholism and its Treatment." By J. E. USHER, M.D. London: Baillière & Co. New York: Putnam's Sons. 1892.

WALDIE. "The True Story of the Introduction of Chloroform into Anæsthetics." By DAVID WALDIE, with remarks by George Waldie. Edinburgh: Oliver & Boyd. 1870.

WETHERED. "Medical Microscopy." By FRANK J. WETHERED, M.D. London: H. K. Lewis, Gower Street. 1892.

WHITE. "Development of the Work of the British Medical Association." By JOSEPH WHITE, F.R.C.S. Edin. London. 1892.

WILLIAMS. "Observations on the General Pathology of Cancer." By R. ROGER WILLIAMS, F.R.C.S. Manchester: John Heywood. 1892.

WILSON. "Micro-organism of Diphtheria." London. 1891.

——— "Clinical Observations on Diphtheria and Croup." By ALBERT WILSON, M.D. Edinburgh: Oliver & Boyd. 1892.

WRIGHT. "Some Principles involved in the Nature and Treatment of Inebriety." By T. L. WRIGHT, M.D. Reprint from the *Alienist and Neurologist*. St. Louis, U.S.A. 1892.

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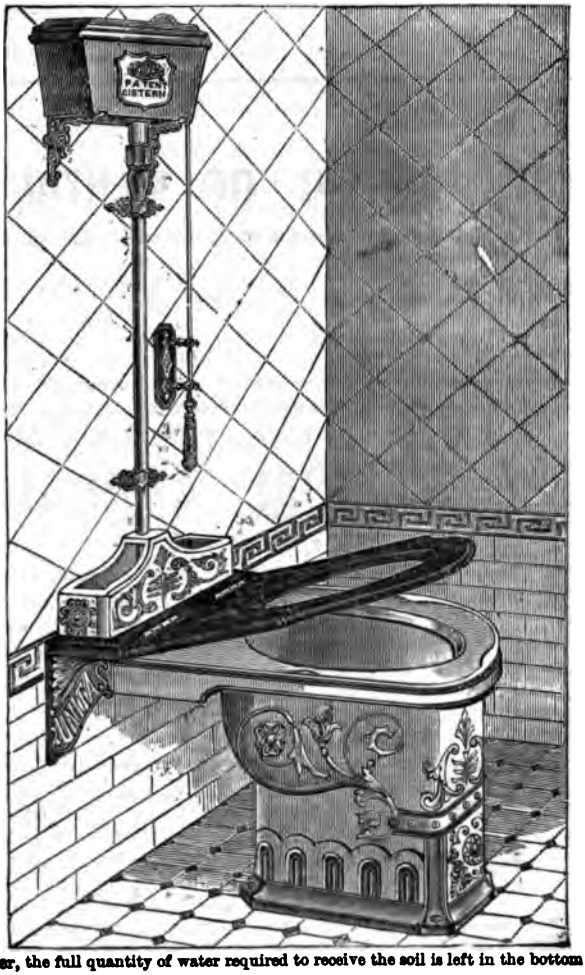

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